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THE EFFECT OF A MOTION PICTURE FILM ON CHILDREN'S ATTITUDES TOWARD GERMANS

RUTH C. PETERSON AND L. L. THURSTONE

The University of Chicago

This is one of several experiments that have been planned for the purpose of investigating the effect of motion pictures on the social attitudes of children. This major problem takes several forms, each of which is the object of several parallel experiments. The present study was undertaken to determine whether the picture "Four Sons" had any measurable effect on the attitudes of high school children especially as regards their attitude toward the Germans and also as regards their attitude toward war.

The general plan for this experiment was similar to that of several other experiments in this study. The children were given certain schedules to be filled in which were intended to measure their attitudes toward Germans and toward war. Some time later they were shown the film "Four Sons." The morning after seeing the film they were again asked to fill in schedules similar to the first set. By this arrangement it was possible to ascertain whether the moving picture had changed the attitudes of the children.

The experiment was carried out in Genoa, Illinois, on one hundred thirty-three high school students.[1] They filled in the first set of schedules on April 17, 1930. They were shown the film "Four Sons" in the Genoa theater on April 29, 1930. The following day in the high school they filled in a duplicate set of schedules. We shall compare the first and second set of schedules in order to ascertain the effect of the film on the attitudes of the children. Genoa, Illinois, is a

[1] We wish to acknowledge the generous interest and assistance of Mr. Cloy S. Hobson, Superintendent of Schools in Genoa, Illinois, and the assistance of the Fox Film Corporation in arrangements that facilitated these experiments.

town with population of about 1200, and the experimental group consisted of almost the entire high school. The children in the experimental group were in Grades VII to XII inclusive.

Three schedules were filled in twice by the students. These schedules were (1) a statement scale for measuring attitude toward the German people, (2) a paired comparison schedule of nationality preferences, and (3) a statement scale for measuring attitude toward war. These schedules were as follows.

A Study of Attitude toward German People

Write your name here.....

Boy or girl.....Age.....Grade.....

Father born in.....Mother born in.....

(Name of Country) (Name of Country)

This is a study of attitudes toward German people. On the other side of this page you will find a number of statements expressing different attitudes toward the Germans.

√ Put a check mark if you agree with the statement

× Put a cross if you disagree with the statement

Try to indicate either agreement or disagreement for each statement. If you simply cannot decide about a statement you may mark it with a question mark.

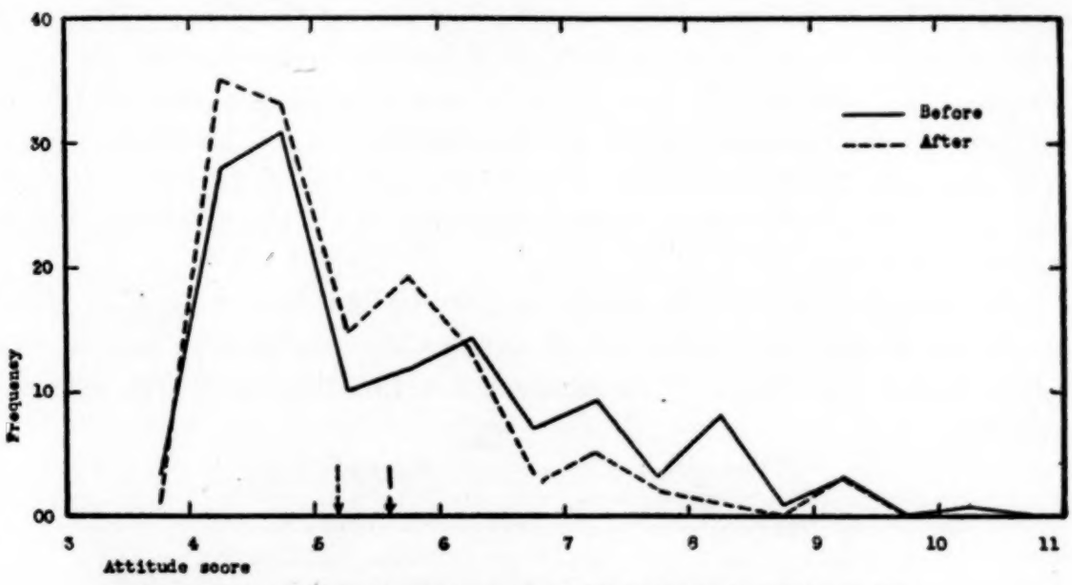
This is not an examination. There are no right or wrong answers to these statements. This is simply a study of people's attitudes toward German people. Please indicate your own convictions by a check mark when you agree and by a cross when you disagree.

√ Put a check mark if you agree with the statement

× Put a cross if you disagree with the statement

(4.6) 1. The Germans as a race are very pleasant people.
(9.1) 2. I'm prejudiced against people of German descent.
(6.4) 3. German people are no better and no worse than any other people.
(1.8) 4. I admire the German people unreservedly.
(12.3) 5. The German people are the most despicable people in the world.
(4.1) 6. Some of our best American citizens are of German descent.
(.7) 7. The German people are the finest in the world.
(10.6) 8. The German soldiers were, almost without exception, cruel and brutal.
(8.4) 9. Germans are slow and unimaginative.
(3.7) 10. The more I know about the German people the better I like them.
(11.5) 11. If Germany had been wiped out by the war the world would be better off.
(7.4) 12. I don't object to German people but I don't like the German Government.
(4.8) 13. The German people were not responsible for and did not want war.
(9.7) 14. I don't trust the Germans.
(9.1) 15. The German military officer is typical of the German people.
(9.0) 16. German parents are harsh to their children.
(11.9) 17. I hate all the Germans.

(4.0) 18. The rank and file of the German army were kindly admirable young men.
(2.7) 19. The Germans are the most desirable class of immigrants.
(2.4) 20. I'd rather have my sister marry a German than any other foreigner.
(5.7) 21. The arrogance of the German officer is not typical of the German people.
(7.0) 22. I have no particular love nor hate for the Germans.
(10.9) 23. There is nothing about Germans that I could ever like.
(7.8) 24. I suppose Germans are all right but I've never liked them.
(3.0) 25. German home life is ideal.
(10.4) 26. The people of Germany are gluttonous, militaristic and overbearing.
(.5) 27. Germans are superior to any other nationality.



Frequency distributions of attitude scores of 130 high school students on the scale "Attitude toward Germans" before and after seeing the film "Four Sons".

FIG. 1.

In front of each of the above statements is recorded its scale value. These scale values are of course omitted from the schedules actually used with the subjects. The score of each subject is the median scale value of all the statements he has endorsed.[1]

In Figure 1 we have drawn two frequency distributions to show the effect of the film. The two mean scores are shown to be 5.6 before seeing the film and 5.2 after seeing the film. This is a shift

[1] This list of twenty-seven statements was used in the experiment here described. Two parallel forms of this scale have since been prepared for publication with twenty statements in each form. These are published by The University of Chicago Press.

in the direction of friendliness toward the Germans. The difference between these two mean scores is .38; $\sigma_1 = 1.43$ and $\sigma_2 = 1.11$. The probable error of the difference is 0.0708 so that the ratio of the difference to the probable error is 5.37. We seem to be justified therefore in concluding that the film "Four Sons" had a significant effect in making children more friendly toward the Germans. It is of some interest to note the correlation between the scores before and after seeing the film. This correlation was +.586. This correlation is determined in part by the reliability of the scale and in part by the fact that the effect of the film was of course not the same for all children.

A statement scale for measuring attitude toward war was also given to these children before and after seeing the film. The scale was adapted from the scale published by the University of Chicago Press. The adaptation was made by simplifying the statements so as to make them more suitable for school children. This scale showed a slight shift in the direction of pacifism but the shift was not large enough to be trustworthy when compared with the probable errors of the mean scores.

In addition to the statement scales the children were also given a paired comparison schedule of nationality preferences before and after seeing the film. The instructions for this schedule were as follows.

A Study of Nationality Preferences

Write your name here.....
Boy or girl.....Age.....Grade.....
Father born in.....Mother born in.....
(Name of Country) (Name of Country)

This is a study of attitudes toward nationalities. You are asked to underline the one nationality of each pair that you would rather associate with. For example, the first pair is:

Englishman-Norwegian

If, in general, you prefer to associate with Englishmen rather than with Norwegians, underline Englishman. If you prefer, in general, to associate with Norwegians, underline Norwegian. If you find it difficult to decide for any pair, be sure to underline one of them anyway. If two nationalities are about equally well liked, they will have about the same number of underlinings in all of the papers. Be sure to underline one of each pair even if you have to guess.

Englishman-Norwegian
Swede-Belgian
Russian-Norwegian
Norwegian-Italian
Pole-Russian
etc.

The complete schedule contains a comparison of every nationality in the list with every other nationality. There were fifteen nationalities in the list. This device is quite sensitive in revealing affective values and by means of the law of comparative judgment the scale

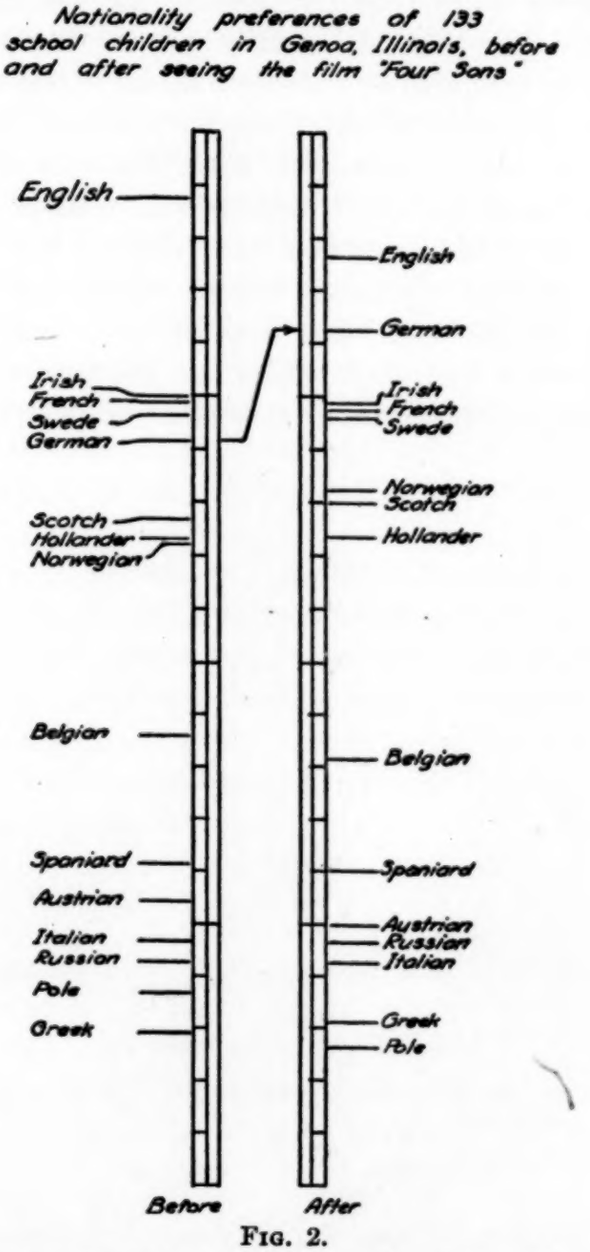


FIG. 2.

value of each nationality was determined. The detailed procedures for these calculations are not here given because they have been previously described.[1]

[1] Thurstone, L. L.: Experimental Study of Nationality Preferences. *Journal of General Psychology*, July-October, 1928.

In Figure 2 we have shown the scale values for all these nationalities before and after seeing the film. It is clear that the largest shift in scale value is found for the Germans. This particular shift is indicated by the arrow in the figure. It shows quite clearly that the film made the children more friendly toward the Germans. The shifts in scale value for the other nationalities are smaller and most of them are of the order of magnitude which would be expected by the chance errors in the proportions by which the scale values were determined.

This study in conjunction with several others of the same kind with different films shows quite conclusively that the social attitudes of children are affected in a measurable way by motion picture films and that international attitudes can be guided by the films. The permanence of the effect of films is being investigated as a separate study in which several attitude scales and comparison schedules have been given to the experimental groups after an interval of four or five months.

UNIQUENESS OF FACTOR PATTERNS

KARL J. HOLZINGER AND FRANCES SWINEFORD

University of Chicago

1. *Introduction.*—It is the purpose of this article to review briefly the principles of factor pattern formation and fitting, and to consider especially the uniqueness of the determined coefficient in such patterns.

For the sake of completeness and clarity we shall begin with a résumé of the ideas in pattern formation which are common to the work of such men as Spearman, Kelley, and the writer.

2. *General Theory of Factor Patterns.*—The term "factor pattern" has been adopted by the writer to describe such factor formations as Spearman's four variable "g" system, Kelley's six factor analyses and any others where the original variables are resolved into linear functions of a number of independent factors. All variables (and factors) are measured in units of their respective standard deviations for simplicity.

The measured variables (in σ units) will be designated as x_1, x_2, . . . x_n and the independent (uncorrelated) factors as u_1, u_2, . . . u_m. The coefficient of any factor u_j for variable x_i is written in the form k_{ij}. Thus the most general factor pattern may be written:

$$\left.\begin{aligned} x_1 &= k_{11}u_1 + k_{12}u_2 + \cdots + k_{1m}u_m \\ x_2 &= k_{21}u_1 + k_{22}u_2 + \cdots + k_{2m}u_m \\ &\cdots\cdots\cdots\cdots\cdots\cdots\cdots \\ x_n &= k_{n1}u_1 + k_{n2}u_2 + \cdots + k_{nm}u_m \end{aligned}\right\} \qquad \text{(A)}$$

The following shorter form, however, is used to save space:

$$\left.\begin{array}{llll} x_1 & k_{11} & k_{12} \ldots & k_{1m} \\ x_2 & k_{21} & k_{22} \ldots & k_{2m} \\ \cdots & \cdots & \cdots & \cdots \\ x_4 & k_{n1} & k_{n2} \ldots & k_{nm} \end{array}\right\} \qquad \text{(B)}$$

Thus Professor Spearman's two-factor pattern is written in the form:

$$\left.\begin{array}{llllll} x_1 & k_{11} & k_{12} & 0 & 0 & 0 \\ x_2 & k_{21} & 0 & k_{23} & 0 & 0 \\ x_3 & k_{31} & 0 & 0 & k_{34} & 0 \\ x_4 & k_{41} & 0 & 0 & 0 & k_{45} \end{array}\right\} \qquad \text{(C)}$$

As a further simplification the specific factor coefficients k_{12}, k_{23}, etc. will be written in a single column separated by a double line from the others:

D

	u_1	Spec.
x_1	k_{11}	k_{12}
x_2	k_{21}	k_{23}
x_3	k_{31}	k_{34}
x_4	k_{41}	k_{45}

Here u_1 is Professor Spearman's "*g*."

As a numerical example we may cite one of Professor Kelley's six-factor patterns:

E

	u_1	u_2	u_3	u_4	u_5	u_6	Spec.	Rel.
x_1	.40	.69	...	.....	.09	.38	.36	.28
x_2	.38	.70	...	.20	...	.....	.47	.33
x_3	.23	−.15	.60	−.17	...	.66	.03	.30
x_4	.21	.....	.63	.....	.31	.....	.16	.66
x_5	.66	−.09	...	.56	...	.....	.36	.33
x_6	.62	.....	.25	.46	...	.....	$\sqrt{-.03}$	.61
x_7	.59	.....	...	.52	.36	.....	.32	.39
x_8	.39	.....	...	.46	.64	.....	.24	.41
x_9	.65	.....	.14	.....	.48	−.31	.33	.35

In this factor pattern, u_1, u_2, etc. correspond to Kelley's α, β, etc. There are also two specific factors for each variable, the second arising from the unreliability of the test and being calculated from the formulae $\sqrt{1 - r_{11}}$, $\sqrt{1 - r_{2II}}$, etc.

We shall next give certain important properties of factor patterns, illustrating each with Professor Kelley's data.[1]

(1) The coefficients k_{ij} express the correlation between the variable x_i and the factor u_j to which they are attached. This may be shown as follows:

$$x_i = k_{i1}u_1 + \cdots k_{ij}u_j + \cdots k_{im}u_m$$

Multiplying both members of this expression by u_j, summing and dividing by N gives $r_{x_iu_j} = k_{ij}$, since all other summed products

[1] Kelley, T. L.: "Cross Roads in the Mind of Man." P. 105.

vanish because of their independence. As examples from Professor Kelley's data we may cite the coefficient .40 which is the correlation between x_1 and u_1, and .69 which is the correlation between x_1 and u_2. These coefficients thus express the degrees of dependence of x_i upon the various factors and are directly comparable with one another as "measures of saturation" of x_i with these factors.

(2) The sums of the products of coefficients under common factors produce (or approximate) the original correlations. Thus in Professor Kelley's example $r_{12} = .40 \times .38 + .69 \times .70 = .635$, the original correlation was .633, so that the agreement is excellent for this correlation. It should be noted that none of the other coefficients in the lines x_1 and x_2 enter into the correlation because no others are common to a given factor such as u_1 and u_2.

(3) The use of the standard deviations must throughout bring the sums of the squares of the coefficients in a line equal to unity. Thus for Professor Kelley's first line: $(.40)^2 + (.69)^2 + (.09)^2 + (.38)^2 + (.36)^2 + (.36)^2 = .9966$ which is very close to one. These squared coefficients may be interpreted as the portions of variance in x_i attributable to the various factors.

(4) Where reliability coefficients are available specific factors with the coefficients $\sqrt{1 - r_{11}}$, $\sqrt{1 - r_{211}}$, etc. may be taken into account. These are the familiar formulae using unit standard deviation.

(5) In determining the coefficients of a factor pattern only a limited number of the total nm can be used because only a certain number of conditions are available for such determination. Thus for nv ariables there will be C_2^n correlations and possibly n reliability coefficients. There are in addition n expressions of the type $\sum_{j=1}^{m} k^2_{ij} = 1$. The total number of conditions is then $2n + C_2^n$ with reliability coefficients and $n + C_2^n$ without reliability coefficients. The number of factor coefficients cannot exceed these maximum numbers if a determinate solution is required.

In Professor Kelley's problem there are nine variables and nine reliability coefficients. The number of intercorrelations of the variables is $C_2^9 = 36$. He may therefore fill up to 54 "cells" of the possible two hundred sixteen (specifics are all different for each variable). By count he has filled only forty-nine so that he has kept well within this limit.

(6) Methods of determining the values of the factor coefficients from the data consisting of C_2^n intercorrelations, n reliability coeffi-

cients and n conditions are varied. Two such methods may be outlined as follows:

(*a*) *Solution by Formula for Special Patterns.*—An example of this method is furnished by the Spearman pattern D. The appropriateness of the pattern is first determined by the familiar tetrad criterion. If such a pattern is justified, the general solution of the coefficients is very simple: $k^2_{11} = \frac{r_{12}r_{13}}{r_{23}}$; $k_{12} = \sqrt{1 - k^2_{11}}$, etc.

Another solvable pattern may be written in the form:

F

x_1	k_{11}	...	...	...	k_{1m}
x_2	k_{21}	...	...	...	k_{2m}
x_3	k_{31}	k_{32}	...	...	k_{3m}
x_4	k_{41}	k_{42}	...	...	k_{4m}
x_5	k_{51}	k_{52}	k_{53}	...	k_{5m}
x_6	k_{61}	k_{62}	k_{63}	...	k_{6m}
. . .	. . . . .	. . . . .	. . . . .	. . . . .	. . . . .
. . .	. . . . .	. . . . .	. . . . .	. . . . .	. . . . .
. . .	. . . . .	. . . . .	. . . . .	. . . . .	. . . . .

The solutions are: $k^2_{11} = \frac{r_{12}r_{13}}{r_{23}}$, $k^2_{21} = \frac{r_{12}r_{23}}{r_{13}}$, $k^2_{31} = \frac{r_{13}r_{23}}{r_{12}}$, etc.

When all the coefficients of u_1 are thus determined we next find those for u_2: $k^2_{32} = \frac{(r_{34} - k_{31}k_{41})(r_{35} - k_{31}k_{51})}{(r_{45} - k_{41}k_{51})}$, etc. and $k_{1m} = \sqrt{1 - k^2_{11}}$, etc.

Such a pattern as the above is very rare, however, and criteria for its determination are uncertain. Spearman's pattern, on the other hand, is common and its criteria and probable error known.

(*b*) *Solution by Approximation in Estimating.*—The next method of determining the factor coefficients is to first make a preliminary estimate of how many factors u_1, u_2, etc. are needed and where the coefficients occur. This may be done by uneven triples of tetrads as follows: For four variables x_1, x_2, x_3, x_4 if we find t_{1234} = large, t_{1423} = −large, t_{1342} = small we have evidence of an extra factor between x_1 and x_2 or between x_3 and x_4 or both. Thus:

$$t_{1234} = r_{12}r_{34} - r_{13}r_{24} = \text{large} + \text{value}$$
$$t_{1423} = r_{14}r_{23} - r_{12}r_{34} = \text{large} - \text{value}$$
$$t_{1342} = r_{13}r_{24} - r_{14}r_{23} = \text{small value}$$

Any of the three following patterns would produce such tetrads.

	u_1	u_2	s
x_1	k_{11}	k_{12}	
x_2	k_{21}	k_{22}	
x_3	k_{31}		
x_4	k_{41}		

or

G

	u_1	u_3	s
x_1	k_{11}		
x_2	k_{21}		
x_3	k_{31}	k_{32}	
x_4	k_{41}	k_{42}	

or

	u_1	u_2	u_3	s
x_1	k_{11}	k_{12}		
x_2	k_{21}	k_{22}		
x_3	k_{31}	...	k_{33}	
x_4	k_{41}	...	k_{43}	

In the subsequent analysis each of these three was tried with certain problems with widely varying coefficients, but with the general fits about equally good.

When the preliminary position of the coefficients has been determined by the above and similar methods the numerical value of the coefficients is next found. Professor Kelley accomplishes this by successive approximations based on the method of least squares. The writer has used successfully a combination of direct solution on parts of the pattern that resemble F and the remainder of the coefficients by trial and error, guided by the fact that the original correlation must be closely reproduced by that pattern coefficient and that the squared sums of coefficients on a line must equal unity. Miss Swineford, who performed all the calculations in the later part of this study, was able to secure good fits to nine variable and six to eight factor problems like Professor Kelley's in about two to three hours for each pattern selected.

(7) *Goodness of Pattern Fit.*—The adequacy of the coefficient values is of course determined by the extent to which the pattern coefficients reproduce the original correlations. It is also necessary that

$$\sum_{j=1}^{m} k^2_{ij} = 1 \tag{1}$$

The writer has devised a simple formula which incorporates both of these principles and serves as an additional check. The proof is obvious and is left to the reader.

$$2\sum r + n = \left(\sum_{i=1}^{n} k_{i1}\right)^2 + \left(\sum_{i=1}^{n} k_{i2}\right)^2 + \ldots + \left(\sum_{i=1}^{n} k_{im}\right)^2 \tag{2}$$

when Σr is the sum of the original correlations.

Two other formulas used in checking the fit will be

M_D = mean difference between original correlations and those produced by the pattern (3)

σ_D = standard deviation of differences between original correlation and those produced by the pattern (4)

We shall next illustrate these checks on Professor Kelley's data given in E. From the original correlations (reproduced in a later section) $2\Sigma r + 9 = 31.63$. Adding the coefficients in each column of Table E, squaring, and then adding these quantities gives 31.57, showing excellent agreement by the use of formula (2). (Note that the coefficients in the specific columns must be squared separately since each really belongs in an individual column.)

Formula (3) applied to Kelley's data gives $M_D = -.001$ while formula (4) gives $\sigma_D = .024$. These checks also indicate an excellent fit, but there is some doubt how to interpret the magnitude of σ_D. The maximum difference in the paired correlations is only .05, however, which approximates the size of the probable error of a single correlation coefficient. A value of $\sigma_D = .024$ thus suggests a good fit.

(8) *Negative Coefficients.*—It is apparent by inspection that we may change the sign of all the coefficients in a column without changing the goodness of fit. As an illustration we shall re-write the general factor portion of Professor Kelley's pattern E, changing the signs of factors (say) u_2, u_4, and u_6:

E^1

	u_1	u_2^1	u_3	u_4^1	u_5	u_6^1
x_1	.40	−.69	...	.....	.09	−.38
x_2	.38	−.70	...	−.20		
x_3	.23	.15	.60	.17	...	−.66
x_4	.21	.....	.63	.....	.31	
x_5	.66	.09	...	−.56		
x_6	.62	.....	.25	−.46		
x_7	.59	.....	...	−.52	.36	
x_8	.39	.....	...	−.46	.64	
x_9	.65	.....	.14	.....	.48	+.31

The fit for E^1 is exactly the same as for E, but we must attach new meaning to the factors u_i^1 *e.g.*, $u_i^1 - u_i$. In the subsequent fitting we have attempted to keep as many coefficients positive as is possible.

3. *Kelley's Seventh Grade Data Fitted with Various Patterns.*—The factor pattern in Table E was the result Professor Kelley obtained upon fitting by his least square method a set of correlations found for a group of one hundred forty seventh grade children taking nine tests. For the convenience of the reader these correlations will be reproduced in Table H (two decimals for brevity).

H

	x_1	x_2	x_3	x_4	x_5	x_6	x_7	x_8	x_9
x_1 = Reading speed	.92								
x_2 = Reading power	.63	.89							
x_3 = Arith. speed	.24	−.06	.91						
x_4 = Arith. power	.06	.07	.42	.56					
x_5 = Memory—words	.20	.31	.04	.15	.89				
x_6 = Memory—numbers	.23	.30	.23	.29	.72	.63			
x_7 = Memory—meaningful symbols	.30	.33	.02	.25	.67	.63	.85		
x_8 = Memory—meaningless symbols	.20	.25	.06	.28	.47	.50	.69	.83	
x_9 = Space—power	.20	.28	.03	.38	.44	.44	.54	.55	.88

The values in the principal diagonal are reliability coefficients.

It might be assumed that since Professor Kelley took a determinate number of factors, and secured the excellent fit of his six general factors as exhibited in Table E that nothing more need be said about the matter. The writer has long suspected, however, that many equally good fits could be obtained for a quite different arrangement of factors, such new patterns being also determinate in the sense that the coefficients are not in excess of $2n + C_2^n$ in number.

Instead of beginning with the mathematical aspects of these differences in fit, we shall first produce several examples of factor patterns fitted to the correlations of Table H.

In our first illustration we shall take the same number of general factors (six) as does Kelley, fitting by direct solution for all coefficients of u_1 and by trial and error for the balance. Using the uneven triples of tetrads to determine extra factors, we get much the same preliminary set up as Professor Kelley, except that we have tried to keep the number of coefficients at a minimum. The result of the fitting is shown in Table I.

The goodness of fit is given by formulas (1) to (4). By formula (1) none of the sums of squares is in error by more than .003; by formula

I

	u_1	u_2	u_3	u_4	u_5	u_6	Spec.	Rel.
x_1	.30	.89	...	...	...	.....	.20	.28
x_2	.42	.56	...	...	...	.63	.06	.33
x_3	.06	.25	.84	...	...	−.35	.13	.30
x_4	.28	...	.48	...	.30	.....	.41	.66
x_5	.62	...	...	.71	...	.....	.05	.33
x_6	.64	...	.22	.44	...	.....	$\sqrt{-.02}$	.61
x_7	.83	...	...	.23	...	.....	.33	.39
x_8	.83	...	...	...	...	−.10	.36	.41
x_9	.65	...	...	...	.60	.....	.31	.35

(2) we get 31.63 indicating a better fit than Kelley; by formula (3) $M_D = .002$; and by formula (4) $\sigma_D = .025$. The maximum differences in correlation are .05 as with Kelley, so that the whole general fit appears at least as good as his.

There is considerable doubt whether our six factors are the same as Kelley's six, but matching them up as the coefficients suggest gives the following double table which facilitates comparisons. Only the six factor portion is reproduced here. The columns headed H are my values; those headed K are Kelley's.

J

	u_1		u_2		u_3		u_4		u_5		u_6	
	H	K	H	K	H	K	H	K	H	K	H	K
x_1	.30	.40	.89	.69	...	...	...	.....	...	.09	.....	.38
x_2	.42	.38	.56	.70	...	...	...	.20	...	...	.63	.....
x_3	.06	.23	.25	−.15	.84	.60	...	−.17	...	...	−.35	.66
x_4	.28	.21	...	.....	.48	.63	...	.....	.30	.31	.....	.....
x_5	.62	.66	...	−.09	...	...	.71	.56	...	...	.....	.....
x_6	.64	.62	...	.....	.22	.25	.44	.46	...	...	.....	.....
x_7	.83	.59	...	.....	...	...	.23	.52	...	.36	.....	.....
x_8	.83	.39	...	.....	...	...	...	.46	...	.64	−.10	.....
x_9	.65	.65	...	.....	...	.14	...	.....	.60	.48	.....	−.31

A few of the outstanding differences between Kelley's and our values in this table are that we have used only fourteen cells in this

part of the pattern whereas he has used twenty-two. Thus with two-thirds as many coefficients we secure as good a fit as he does. The coefficients of u_1 are fairly similar, but large differences occur for the u_2 factor in x_3, the u_4 and u_5 factor in x_8, and for the u_6 factor in x_1, x_2 and x_9.

In our next pattern we shall again be guided by the uneven triples of tetrads in determining the preliminary pattern but with one important difference. When a set like t_{1234} = large, t_{1423} = large and t_{1342} = small occurs we shall use only one extra factor such as between x_3 and x_4, being parsimonious in the allocation of all such extra factors, at least at the start. The result of this fitting is presented in Table K.

K

	u_1	u_2	u_3	u_4	u_5	u_6	Spec.	Rel.
x_1	.68	...	.....	...	.....	.68	.00	.28
x_2	.89	...	−.15	...	.....	...	.28	.33
x_3	.12	...	.89	.23	.....	.23	.00	.30
x_4	.11	.10	.37	.32	.55	...	.00	.66
x_5	.33	...	−.20	.79	−.12	...	.32	.33
x_6	.34	...	.....	.73	.....	...	$\sqrt{-.02}$	.61
x_7	.39	.35	−.18	.65	.....	...	.34	.39
x_8	.29	.62	−.10	.52	.10	...	.27	.41
x_9	.30	.31	−.10	.45	.37	...	.58	.35

Here we have filled two more cells than Professor Kelley.

In applying the tests of goodness of fit, we find from (1) that none of the sums of the squares is in error by more than .005; formula (2) gives 31.66 which is again closer to 31.63 than Kelley's value; we also find from (3) and (4) that M_D = .001, and σ_D = .019. Our maximum difference in correlation is only .04, whereas Kelley's is .05. All of these results indicate a better fit than Kelley has attained in E.

The factor pattern coefficients in K are so different from Kelley's that we shall not try to match as in Table J because we are less sure that the six factors are the same. Some of the points of difference in Tables E and K are as follows:

(*a*) Kelley obtains a large extra factor between x_1 and x_2 and calls it "verbal." In our Table K, there is *no* extra factor between x_1 and x_2.

(*b*) The extra correlation for x_3 and x_4 is produced in Kelley's pattern by the coefficients .60 and .63, whereas in ours it is produced by .89 and .37 and also by .23 and .32.

(*c*) The extra correlation between x_5 and x_6 in Kelly's pattern is produced by coefficients of .56 and .46 and in our pattern by a larger pair 79. and .73.

(*d*) The extra correlation between x_4 and x_9 is produced in Kelly's pattern by two products, and in our pattern by four products.

It is also of interest to compare our patterns I and K. In pattern I none of the five factors u_2 to u_6 is present in more than three variables, whereas in pattern K, u_3 and u_4 run through seven variables (in u_4 these seven coefficients are all substantial in size). The general disagreement in position and value of coefficients in these two tables is very striking no matter how the columns be arranged.

Taken together, Tables E, I, and K show such wide variation in coefficient positions and values that even where the number of factors is fixed at six, the particular fit obtained may be of very little value because it is not unique as indicated by these examples.

We shall next attempt a fit using only five general factors. The method employed is similar a that used in obtaining pattern I, but with greater parsimony employed.

The result is shown in Table L.

L

	u_1	u_2	u_3	u_4	u_5	Spec.	Rel.
x_1	.29	.91	−.06	.05	...	.06	.28
x_2	.35	.56	−.28	.11	.07	.60	.33
x_3	.04	.27	.83	.13	.36	.00	.30
x_4	.32	...	.38	.56	...	.06	.66
x_5	.68	...	−.22	...	.58	.21	.33
x_6	.68	...	.....	.07	.44	$\sqrt{-.03}$	.61
x_7	.91	...	.....	...	...	.14	.39
x_8	.77	...	.....	...	...	.49	.41
x_9	.66	...	.....	.23	...	.62	.35

The five factors do not, of course, correspond generally to those in Tables E, I, and K except that u_1 and u_2 appear very similar.

The fit of the table is shown by a maximum error of .005 in the squared sums on a line; by a value from formula (2) of 31.63 showing perfect agreement to two decimals; by $M_D = .001$; and by $\sigma_D = .028$. The fit is again as good as Kelley's in Table E.

It will be noted that no common factor of any magnitude is present in the last four variables except u_1, which is quite in contrast with Table K, but that the extra factor between x_1 and x_2, missing in Table K, has been reinserted with very large coefficients in Table L.

In addition to the above pattern we have fitted Professor Kelley's correlation with seven and eight factor patterns, but space does not warrant introducing them here. It would seem that using a smaller number of factors is generally better and that the pattern with the smallest number is best of all. If this be true our pattern L is superior to Kelley's and the rest.

We have also fitted Professor Kelley's third grade data with various patterns giving results similar to those found above. If space permitted we could readily show that the agreement in coefficients Professor Kelley gets between his seventh and third grade patterns is little indication of their correctness. Patterns for these grades quite different from his may also be brought into agreement.

4. *Conclusions.*—It has been shown that Professor Kelley's seventh grade correlations may be fitted by a number of patterns varying as to number of factors, position of coefficients, and numerical value of these constants. These variations are brought about chiefly by differences in the preliminary estimates of necessary additional factors. The uneven tetrad tripled analysis leads to ambiguity as shown above. Thus when t_{1342} is small and the other two tetrads are large we may have extra factors between x_1 and x_2, between x_3 and x_4, or both. Carrying this analysis through all of the variables leads to markedly different preliminary patterns and eventual fits.

Some of the variation in pattern is due to the methods of fitting. We have used chiefly direct solution (as for all coefficients of u_1) and trial and error for the balance. The test of the value of these methods is that they produce as good or better fits than those secured by Professor Kelley. In any event the method of getting the values for the coefficients is of little importance as long as the fit is obtained. We found the methods described easier to handle than the least square procedure, but the latter would probably bring results similar to ours if the same preliminary values are employed. It may be said that a certain amount of trial and error procedure must have been used by

Professor Kelley himself, since his original guesses as to position of coefficients are often changed later on.

The point of this paper has been to illustrate lack of uniqueness in pattern fitting by procedures thus far employed. Some other procedures may possibly lead to better results, but the analysis represented by Professor Kelley's work cited above is clearly inadequate.

THE PROBLEM TEST IN ELEMENTARY EDUCATIONAL PSYCHOLOGY[1]

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Introduction

Although tests are usually given for the purpose of obtaining grades teachers have not lost sight of the fact that tests are a very useful means of stimulating study. No device motivates more powerfully the review and organization of subject-matter in large units. A test consisting of problems calling for the assimilation of facts scattered in several chapters is especially well adapted to this purpose.

The teacher in service needs his psychology when a classroom difficulty arises that cannot be wisely resolved without it. There is no better way to convince prospective teachers that psychology can help them or to teach them how to search for the information they need than to challenge their powers with problems like those they will encounter in service. Nor are there insurmountable obstacles to distributing the problems in advance of the date of the test. Distribution in advance merely duplicates the situation of the teacher in service who can use books and ask as many questions as he pleases concerning his difficulties. The problems can be selected, moreover, in such a way as to cover all or nearly all of the more important ideas in the subject-matter in question.

In view of these facts it is worth while to investigate the effects of the problem test on classes in educational psychology. The following report is concerned with two questions: (1) Does the problem test distributed in advance cause the student to learn as many facts in his review of the subject-matter as the usual test, the nature of which is not known in advance, and (2) Does the solving of problems convince the student that psychology can be useful to teachers, that is, does it give him favorable professional attitudes toward the subject? With the intention of throwing light on these questions a problem test distributed in advance and a test announced in advance without information as to its nature are compared below in respect to their effect on the learning of factual information and on the attitudes of students towards them.

[1] Read before the psychology section of the National Society of College Teachers of Education, in Detroit, February, 1931.

Description of Procedure

The subject-matter with which the investigation was concerned included four chapters of a well known textbook the study of which had been completed before the testing began. One of the chapters treats the methods and subject-matter of educational psychology and the remaining three deal with instincts and emotions and their functioning in the formation of the so-called mental adjustment mechanisms. The subjects were juniors and seniors, members of classes in educational psychology at the University of Alabama during the year 1930. The experimental group, which included both a spring and a summer class, received its test three days in advance of the date when the papers were due. This test consisted of seven practical problems bearing on instincts and emotions, similar in a general way to those found in Whipple's Problems in Educational Psychology. None of the problems related to the first chapter.

The control group A, which also included both a spring and a summer class, was informed simply that it would have a test on the four chapters two days later. On that date it took a test consisting of four problems similar to those of the experimental group, in order that its experience with the subject-matter might differ from that of the experimental group only in respect to the nature of its preparation for the test. The average scores of the two groups on the Otis Group Intelligence Scale, advanced Examination, Form A were nearly the same, being 165.8 for the experimental group and 161.7 for the other. Both groups worked under the same instructors and both made nearly the same average score on the preliminary giving of the achievement test.

The control groups B and C worked in the fall of the year under the same directions as group A, except that, on the date announced for the test, group B took the achievement test for the second time and group C was given its second try on the achievement test as a surprise. The low score of group C on the preliminary test (Table I) is probably accounted for by the fact that it contained a number of students who were very zealously preoccupied with athletic and social activities.

An objective test consisting of 98 completion sentences, word classification groups, and simple recall items was constructed to measure achievement. Part I of the test included seventy-three items relating to the textbook's discussion of instincts and emotions and

TABLE I.—COMPARISON OF THE EXPERIMENTAL AND CONTROL GROUPS IN RESPECT TO MEAN GAIN ON REPETITION OF THE ACHIEVEMENT TEST

Group	Number of cases	Mean score		Mean gain		Difference between gains of experimental and control groups	
		Preliminary test	Retest	Amount	PE	Amount	PE
1. Experimental: Problem test distributed in advance	28	38.8	48.6	9.9	0.86		
2. Control A: Test announced in advance type not described. Wrote one hour, problem test	31	37.3	52.9	15.6	0.30	5.7	1.6
3. Control B: Test announced in advance, type not described	42	33.8	46.6	12.8	1.05	2.9	1.4
4. Control C: Achievement test repeated, without review	29	19.9	23.9	4.0	0.84	5.9	1.2

Part II included twenty-five items on the first chapter. The achievement test was given to each group without warning for the preliminary measurements and it was repeated without warning, except in the case of control group B, two days after the training series had been completed. By correlating random halves and correcting with the Spearman-Brown formula the reliability coefficient of the achievement test was found to be 0.86.

EFFECT ON INFORMATIONAL LEARNING

The performances of the several groups on the first and second givings of the achievement test are recorded in Table I. The experimental group gained 9.9 points on the retest as compared with control group A's gain of 15.6 points. The size of the probable error (1.6) of the difference (5.7) between the two means, however, indicates that the disparity may be accounted for entirely by variations in sampling. By the same method of statistical analysis the slight inferiority (2.9 points) of the experimental group's gain (9.9) in comparison with that (12.8) of control group B is found to be insignificant. The experimental group, on the other hand, gained 5.9 points (PE 1.2) more than control group C which merely repeated the achievement test without additional study. The experimental group's review of the text in its effort to solve the problems doubtless accounts for its significant superiority in this case.

Up to this point the data seem to show that reviewing the text to solve a set of problems can produce a better knowledge of the facts involved than no review at all and about as much a knowledge of the facts as would have been gained by the usual general review for a test the nature of which was not known in advance.

Further evidence pointing in the same direction may be brought to light by comparing scores on different parts of the achievement test. The problems of the experimental group were limited to the subject of instincts and emotions as were the questions which constituted part I of the achievement test. The members of the experimental group did not find it necessary, therefore, to study the first chapter of the text, with which part II of the achievement test was entirely concerned, and presumably few, if any of them did. Control group A, on the other hand, possessing no information as to the nature of their test, presumably studied the first chapter as carefully as the three chapters on instincts and emotions. Control group A, therefore, was prepared for both parts of the achievement test.

TABLE II.—COMPARISON OF THE MEAN GAINS OF THE EXPERIMENTAL GROUP AND OF CONTROL GROUP A ON DIFFERENT PARTS OF THE ACHIEVEMENT TEST

Group	Mean gain (at retest) on parts		
	I and II	I	II
Control A	15.6 (PE 1.3)	8.0 (PE 1.2)	7.6 (PE 0.5)
Experimental	9.9 (PE 0.9)	5.8 (PE 0.9)	4.1 (PE 0.4)
Difference (C − E)	5.7	2.2	3.5
PE of difference	1.60	1.45	0.66

The gains of these groups on both parts of the test are presented in Table II. Control group A gained eight points on part I which compares with a gain of 5.8 points on the part of the experimental group. The probable error (1.45) of the difference (2.2) between the two means is so large that it cannot be considered significant. The difference (3.5) in the gains of the two groups on part II in favor of group A, however, is significant. The experimental group, in other words, did not gain as much on that part of the achievement test which was not covered by the problems, but it did gain practically as much on the other part the subject-matter of which it had to review to solve the problems. These facts emphasize the conclusion drawn

above. They also point out the need of taking pains to select the elements of a problem test in such a way as to cover all of the subject-matter which the instructor desires the class to review.

Effect on Study Habits

The groups that took part in the experiment during the spring were asked if they worked with others in preparing for the tests. In the beginning the instructor had advised them to get help from whom-so-ever they could. Thirteen of the eighteen members of the experimental group followed this suggestion, but only four of the twenty members of control group A did so. When students are confronted with interesting problems which do not yield to their efforts immediately, it seems that they consider it profitable to pool their information and to seek advice from others—a practice the wisdom of which is supported by important experimental evidence.[1] A test of practical problems distributed in advance appears to provoke serious group discussion more effectively than the usual test withheld until the examination hour.

Another important factor is the amount of time spent in preparation for a test. Both groups were asked to report for each day they worked the total number of minutes they used. The experimental group reported an average of nine hours and eighteen minutes used in study where-as group A reported an average of five hours and twenty-five minutes, including one hours writing during the class period. The problem test evidently can be made to demand longer periods of study than students customarily spend in preparation for tests the nature of which they do not know ahead of time.

The Attitudes of Students

The effect of a test on the attitudes of students is a consideration not to be neglected. Nineteen of the twenty-five members of the experimental group, who committed themselves, expressed a decided preference for the problem test, three preferred tests not distributed beforehand, and three replied it made no difference to them. More than three out of four, in other words, preferred to work with problems despite the much greater amount of time and work involved. Is it because they like to know in advance precisely what is expected and

[1] See Watson, G. B.: Do Groups Think More Effectively Than Individuals? *Journal of Abnormal and Social Psychology*, Vol. XXIII, No. 3, (Oct.-Dec. 1928), pp. 328–336.

to have plenty of time to do it or do they consider working with problems more interesting and more valuable?

These questions were answered partially, at least, in the statements of the twenty-five students concerning the advantages and disadvantages of the problem test. The statements are briefly summarized below under the heading, Attitudes of the Experimental Group toward the Problem Test. It is noteworthy in the first place that approximately three times as many statements were concerned with advantages as with disadvantages. Some of the former were motivated, obviously, by a distaste for hurry and the accompanying emotional tension as well as by the desire to be told precisely what to do (*A*, *C*, and *E*). Statements *A*, *B*, *D*, and *F*, on the other hand, indicate an interest in more refined and more productive methods of study or in the application of scientific information to professional questions. Some of the replies may have been influenced by the instructors' discussion of the comparative merits of the two kinds of tests before the experiment began, although the papers were not signed. The clarity, decisiveness, and individuality of many of the original replies, on the other hand, suggest genuine conviction. No objective measurements are available on the question of increased skill in solving problems and yet statement *D* in particular suggests that some, at least, thought they had made gains in this direction.

ATTITUDES OF THE EXPERIMENTAL GROUP TOWARDS THE PROBLEM TEST
(Twenty-five Students)

I. Advantages ascribed (seventy-five statements).
   (*A*) Allows time for more study of a better type and for the preparation of better reports (27).
   (*B*) More conducive to research and to organized constructive thinking (16).
   (*C*) Assists students in selecting and attacking the more important topics (10).
   (*D*) Teaches students how to search for information and to apply it to practical problems (9).
   (*E*) Induces greater interest and reduces nervousness and consciousness of effort (7).
   (*F*) Encourages discussion and learning from others (6).

II. Disadvantages ascribed (twenty-six statements).
   (*A*) Permits neglect of topics not included in the problems (8).
   (*B*) Demands too much time (5).
   (*C*) Knowing what the test is leads to postponement of work until too late to make a thorough study (5).
   (*D*) Encourages some to depend too much on others (5).
   (*E*) Conducive to nervousness or indefiniteness in writing reports (2).
   (*F*) Gives unfair advantage to students with teaching experience (1).

The most significant of the disadvantages that were mentioned, *A*, *B*, *C*, and *D* can be minimized or overcome by care, on the part of the instructor, in the selection of problems and in building up the morale of a class. Finally, for the information of those, who are interested in the grading of problem tests, it may be said that one of the writer's colleagues has a study well under way which seems to prove that answers to essay tests can be scored with a much greater degree of reliability than has been generally conceded. There was, moreover, a correlation of .71, uncorrected for attenuation between the scores of the experimental group on the achievement and on the problem tests.

Conclusion

The conclusions of this report may be summarized briefly as follows: The problem test distributed in advance can be employed to motivate the review of a unit of subject matter in educational psychology (1) with the expectation that helpful habits of study will be exercised and (2) that professionally valuable attitudes will be developed by many students (3) with approximately as great a gain in informational learning as is to be expected from the use of tests the nature of which is not known beforehand.

THE RELATION OF REPORTED PREFERENCE TO PERFORMANCE IN PROBLEM SOLVING[1]

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The theory that interest in a learning activity is necessary to pleasurable effort and that through interest success is more probable is generally accepted by modern educators. A question arises, however, when this theory is assumed to mean that mere expressions or indications of interest in an activity or group of related activities are symptomatic of abilities and consequent achievement in them.

Any expressed interest or preference is considered by some as an unerring guide in the selection of subject-matter and learning exercises. The likes and dislikes, it is maintained, are native and permanent in the individual and are indicative of what is best for his well-being and growth. Pringle[2] says,

> The high place given here to Psychology, and especially Adolescent Psychology, finds support in the doctrine of the "new Education," which teaches that the nature of youth, his active interests and the conditions which surround him should determine each stage of his development, rather than any ideals as to what a mature person should be or do. The question is not what will be of most value to the youth when he grows up, but what is of most interest now.

On the other hand there are educators who take the position that children's interests are evanescent and changeable, and consequently should not be the sole guide in the selection of subject-matter. Counts says, "Nothing should be included in a curriculum merely because it is of interest to children, but whatever is included should be brought into the closest possible relation with their interests." He expresses surprise and regret that, "The thought is sometimes suggested that the child should serve as an expert consultant on curriculum content."[3]

A second implication of the question of interest and achievement has to do with methods of teaching. The great emphasis placed

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[1] For complete account of this study see Bowman, Herbert Lloyd: "The Relation of Reported Preference to Performance in Problem Solving. *University of Missouri Bulletin*, Vol. XXX, No. 36. Columbia: University of Missouri, 1929, pp. 52.

[2] Pringle, Ralph W.: "Methods with Adolescents." 1927, pp. 13–14.

[3] Counts, George S.: Some Notes on the Foundation of Curriculum Making. *Twenty-sixth Yearbook of the National Society for the Study of Education*, Pt. II, Chap. VI, pp. 75–90.

upon children's interests and preferences as a guide in the choice of learning activities has led consequently to a like importance being attached to "making the recitation interesting" to the learner. Devices for motivating school work are enumerated and emphasized in numerous books on teaching technique. The so-called project method is an outstanding example of a procedure in teaching in which efforts are made to utilize children's interests to the greatest possible extent.

The scientific basis for the stress placed upon the use of children's likes and interests in the selection of subject-matter and methods is the law of effect, formulated by Thorndike. This law may be stated in more than one way, but a commonly accepted statement of it is, the ease of learning is increased if either the result or the action used in reaching the result is satisfying, but decreased if either the result or the action used in reaching it is annoying.

The question of the relation of ability to interest has been studied principally from the standpoint of broad fields of subject-matter. Preferences for certain school subjects or for certain vocational activities have been obtained and compared with achievement in these subjects or fields. A careful review of these studies reveals that there is no general agreement as to the relation interest bears to ability, nor as to the stability of early interests. Since most of these investigations refer to interests and abilities in broad fields where these traits are general and widely operative rather than specific, a condition exists which is favorable to the entrance of many uncontrolled factors into the measurement of the traits. This increases the subjectivity and consequently the difficulty of accurate determination.

In order to throw more light upon the involved question of interests and the part they play in the learning process, it was believed advisable to try to determine the relationship between expressed interest or preference and achievement in a narrow field of activity. For this purpose we endeavored to answer this question, in part, by determining what degree of relationship exists between reported preferences for problems in arithmetic and performance on these problems by pupils of the junior high school level. The usual conception of the term 'interest" is that of preference for some one situation over another, and it is this meaning of interest that we employed, using the expression "reported preference" in order to provide for a greater degree of objectivity. When an individual says he prefers one situation to another we mean that he is more strongly stimulated to respond posi-

tively to it than to a second situation. There is no way of measuring the degree of such stimulation but we can draw an inference as to its nature by observing the reactions of the individual.

The Problem

Our problem was, then:

1. Are pupils more successful in performing problems for which they express a preference?

2. Of what significance is this relationship of reported preference to performance with respect to:

(*a*) Nature and types of problems used?

(*b*) Intelligence and ability of pupils?

In this study the expression "reported preference" rather than the term "interest" was employed, as stated above, since the latter is more subjective and lends itself to varied interpretations. Reported preference means that of several items from which to choose, one item is *reported* as being more preferred or better liked than any of the others.

Procedure

In undertaking the attack of this problem it was obvious that some form of the test technique must be employed. First, it was essential that arithmetic problems be presented to the pupils for actual solution, and second, provision for obtaining reports of preference for certain of these problems over others was necessary. Furthermore, in order to provide differentiation and therefore a basis for preference, the problems of necessity must represent certain definite types throughout the test.

A thorough study of numerous arithmetic texts and standardized tests led the writer to select five types of problems as being distinct as well as representative of what are found in the most widely used text book matter. The five types were: (1) Problems based upon adult activities, (2) problems based upon children's activities, (3) problems whose settings are in the fields of science, (4) problems so stated as to take on the nature of a puzzle, and (5) problems of pure computation only, in which the directions for the correct procedure are given. For the sake of convenience these types were called, respectively, (1) the adult type, (2) the child type, (3) the science type, (4) the puzzle type, and (5) the computation type.

Fifty problems, consisting of two test forms of twenty-five problems each, were selected to make up the items of the research test. Each form consisted of six mimeographed pages. Page 1 contained space for pupil data, also instructions to pupils, while on the remaining five pages there were twenty-five problems to be solved, five problems to a page. One of each of the five types was placed on each page and the positions of the various types were rotated so that none occupied the same location on any two pages. At the bottom of each page, beneath the problems, this incompleted sentence was placed to be filled in by the pupil: "The problem on this page I like best is number ———."

In order that the preference for a problem be based upon the problem situation and not upon the element of difficulty of computation, it was necessary to hold constant this difficulty factor among the problems of one group or page. The very nature of this study precluded the use of problems that were equal in difficulty with respect to *all* factors. The problem situations, the objects named in the problem, the relative positions of the different phrases, the form of the questions used, and the symbols and technical terms employed, all affect the difficulty of a problem. These, however, are the very elements which typify the various problems used for comparison. The same elements which affect the relative difficulty of problems are likewise the elements which in themselves differentiate them into types.

Efforts were made, however, to make the computation difficulty as nearly equal as possible among the problems of each group or page. This was accomplished in two ways. In the first place, the problems were so selected or constructed that all on one page required like operations for correct solution. Secondly, the same digits were used, as nearly as possible, in the various problems of a page, arranged in different positions or order. This provided for approximately the same combinations of numbers in the steps of the problems of each group. A complete description of the construction of the research instrument used in this study cannot be given here, but this, together with a full account of the investigation is given in the publication referred to at the beginning of this article.

After a careful revision of the test items, following try-out tests, the two forms were administered to five hundred sixty-four pupils of Grades VII, VIII, and IX of the Martha Letts Junior High School, Sedalia, Missouri. From the results obtained, the performance reliability was calculated and found to be: $r = .95 \pm .005$. Likewise the preference reliability was: $r = .94 \pm .004$.

The validity of the research test in measuring performance in problem solving was determined by correlating the results with scores made on certain standard tests. The results of these calculations are found in Table I, and indicate a relatively high degree of validity.

TABLE I.—CORRELATIONS OF RESEARCH TEST WITH CERTAIN CRITERIA

	Research Test
Stanford Arithmetic Reasoning Test	0.82 ± .01
Stevenson's Arithmetic Reading Test	0.72 ± .01
Otis Self-administering Test of MA	0.63 ± .02
Teachers marks in arithmetic	0.60 ± .02

As has been stated, the research test consisted of two forms of twenty-five problems each, constituting in reality one test. There were five hundred sixty-four pupils who took both forms of the test, and the score assigned to each pupil was the sum of the scores on both forms. This provided a possible range of from 0 to 50. The distribution of the performance scores is not shown here, but it was fairly typical and symmetrical.

In order to obtain expressions of preference the subject was asked to indicate, after solving or trying to solve the five problems on each page, which one of the five he liked best. To do this the pupil merely wrote the number of his preferred problem in a blank provided at the bottom of the page for that purpose. The differentiation between the problems of one group was based upon the settings or situations which clothed the problems. These settings, as has been explained, were of five kinds, namely, (1) the adult, (2) the child, (3) the science, (4) the puzzle, and (5) the pure computation. In treating the results of the record of preferences, we dealt with individual and total preferences as that of preferences for types of problems and not as that of preferences for particular problems.

RESULTS

A primary concern in endeavoring to obtain expressions of preference was the efficacy of our testing device in actually getting the preference response. Table II shows the result in this connection. Since there was a total possible preference report per pupil of ten, it can be seen that, had there been a perfect report by each pupil, 5640 preferences would have been indicated. The 5222 actually reported represents 92.6 per cent of the total possible.

The total performance of the five hundred sixty-four pupils on each of the types of problems was determined and these results, together

TABLE II.—TOTAL NUMBER OF PREFERENCES POSSIBLE TOGETHER WITH TOTAL NUMBER ACTUALLY REPORTED

Number pupils	Number preferences possible	Number preferences reported
564	5640	5222

with the total number of preferences by types are shown in Table III. An inspection of this table seems to indicate that there is considerable variation in the types of problems which pupils prefer and in their performance on the problems of the various types. If the subjects had had no distinct preference, and in reporting a choice had merely chosen one at random, then by the laws of chance the number of indicated preferences for each of the types would have been approximately equal. Likewise, since the five problems of each group were of equal computational difficulty, the number of problems of each type correctly solved would have been equal had no factors of difficulty due to the descriptive situations operated. It is evident that the setting or description in which a problem is clothed influences a pupil's preference and likewise his performance. It will be noted that the computation type ranked considerably higher in both preference and performance. It was chosen as a preference more than twice as many times as the puzzle type, which was the lowest in preference, and it scored over twice as much in performance as the science type, which ranked lowest in that respect.

TABLE III.—NUMBER PROBLEMS PREFERRED AND NUMBER PROBLEMS PERFORMED OF EACH OF THE FIVE TYPES OF PROBLEMS

	Types of problems				
	Adult	Child	Science	Puzzle	Computation
Number problems preferred.....	947	1162	799	625	1689
Number problems performed....	2124	2371	1790	2242	3578

The greater number of correct solutions of the computation type of problem may be accounted for in the fact that this type naturally was devoid of the difficulty due to any descriptive setting. An analysis of the results with respect to the intelligence and ability of the pupils

revealed that pupils of lower ability rather consistently selected the computation type as a preference.

In order to treat the preference reports statistically it was necessary to obtain an arithmetic value for each preference, and thus a total preference score for each pupil. This was done by assigning to each of the problem types, upon which the preferences were based, some numerical value. This value or weight was determined by ranking the types according to the total number of preferences reported by all pupils for the various types. For example, the type for which the greatest number of preferences was reported was ranked first and was assigned the value of one; the type for which the second greatest number was reported was given the value of two, etc. We learn from Table III that the computation type ranked first in number of preferences received, the child type second, the adult type third, the science type fourth, and the puzzle type fifth. Based on this order of ranking, then, we assigned values to each of the types as follows:

Computation type	1
Child type	2
Adult type	3
Science type	4
Puzzle type	5

The next step was actually to determine the preference score for each individual. The number of preferences for each type was obtained for the individual. These numbers were then multiplied, respectively, by the assigned value of the type each represented. We shall illustrate with an example. Case 13 of Grade VIII reported three preferences for the adult type, five for the child type, and two for the science type. His preference score was determined thus:

3 times 3	= 9
5 times 2	= 10
2 times 4	= 8
Score	= 27

The distribution of the preference scores was highly symmetrical, showing a median score of 25.8 and a mean of 25.4.

Relation of Reported Preference to Performance

A comparison of the preference scores with those of performance by means of a coefficient of correlation revealed something of the

degree of correspondence between the two variables. When the preference scores were correlated with the complete performance scores a coefficient of correlation of .46 ± .03 was obtained. Since the preference scores were based upon the rank given by the group to the types of problems, we likewise, and by a similar method, obtained performance scores by weighting the score on each type according to that type's rank in performance by the group. By correlating these two sets of derived scores, a correlation of .58 ± .02 was discovered.

Neither of these coefficients of correlation indicate a degree of relationship high enough for reliable prediction, yet they are markedly positive and considerable significance can be attached to them. They mean, in the first place, that there is a positive correspondence between high performance in arithmetic and unusualness of preference, the individual scoring high in performance on the research test tending to prefer the problems not commonly preferred by the group as a whole. They further indicate that mediocrity in performance goes with mediocrity in preference as valued here, and that low performance corresponds with usualness of preference. In short, as the ability to solve arithmetic problems increases, preference agrees less and less with usual preference as determined by the group as a whole.

The coefficients of correlation reported above were obtained when the preference scores were correlated with *total* performance. We were interested to learn what correlation existed between preference and performance only on the problems preferred. Accordingly the preference scores were correlated with actual scores made on the problems that were preferred. We found a coefficient of .22 ± .03 resulting from this correlation. It would seem that one might expect this correlation to be somewhat higher than that between preference and total performance. However, in using the actual scores only on the preferred problems, the range of performance was very narrow since the maximum number of problems preferred was but ten. In this situation it is conceivable that a pupil with a low preference score might easily possess as high a performance score as one with a high preference score. It is probable, then, that the correlation of .22 represents as high or a higher relationship than does the correlation of .46 between preference and total performance.

In order to make more comparable the performance scores on the preferred problems, we weighted each of the preferred problems that was solved and obtained a derived performance score on the preferred problems. These were correlated with the preference scores,

and the result was a coefficient of correlation of .56 ± .02. This coefficient of correlation is probably the best expression of the relation existing between reported preference and performance. It means that when unusualness of preference is given high value and unusualness of performance is valued in the same way, the correlation between reported preference and performance on the items preferred is as indicated above. Reversing the values both in preference and performance would in this particular calculation produce at little difference.

To summarize these relationships we present the various reputed coefficients in Table IV, giving with each coefficient the corresponding Predictive Index as read from Bailor's table.[1]

TABLE IV.—A TABLE SHOWING ALL THE COEFFICIENTS OF CORRELATION BETWEEN REPORTED PREFERENCE AND PERFORMANCE TOGETHER WITH THE CORRESPONDING PREDICTIVE INDICES

	Total performance unweighted	Total performance weighted	Performance on preferred problems unweighted	Performance on preferred problems weighted
Reported preference weighted.....	.46 ± .03	.58 ± .02	.22 ± .03	.56 ± .02
P. I.	.1121	.1854	.0245	.1716

The formula for the Predicative Index (P. I.) is P. I. = $\sqrt{1-r^2}$.

Analyses of preference and performance with respect to the general intelligence and ability of the pupils were made and certain definite tendencies were noted. The most significant result shown in this connection was the tendency for pupils of high ability to perform equally well on all types of problems, and furthermore to indicate no decided preference for any particular type. On the other hand pupils of lower ability more consistently preferred the pure computation type and exhibited a higher relative degree of performance on this type of problem. The computation problem used in the test was one which called for simple directions to be read and followed. We may conclude, then, that pupils of lower ability seem to prefer problems which are not set in complex situations, but rather those which

[1] Bailor, Edwin, M.: Content and Form in Tests of Intelligence. *Contributions to Education*. No. 162, 1924, Teachers College, Columbia University.

necessitate only the following of specific directions. In short, there was a definite tendency for the individual to select as his preference that problem which he believed or knew he would be more nearly successful in solving. We seem to be justified in inferring that the expectation of greater success leads an individual to prefer one problem over others, and that belief in success causes preference rather than that preference is a cause of successful performance.

Conclusions

We draw out of the body of this study certain conclusions of psychological and practical significance.

1. Reported preference within a narrow field can be measured with a remarkable degree of accuracy by employing definite reporting methods in narrowed-down preference situations.

2. Reported preference in the particular connection here studied correlates with performance to the degree of .56.

3. This relation is not large enough to predict performance from reported preference. In this particular field a glib prediction as to the probable success of a pupil, knowing his reported preference, is entirely unwarranted.

4. The relation between reported preference and performance is of enough significance to demand some attention when we select problems for textbooks.

5. The general intelligence of pupils should be used as a basis for the selection of arithmetic problems for the junior high school. Both the relative degrees of performance and reported preference on the various types of problems vary as between levels of intelligence.

6. Of problems involving descriptive situations, the child type is most preferred and most successfully performed by all groups of junior high school pupils. The adult type ranks second, while the science type ranks lowest in both respects. It may be inferred that this ranking is probably due to the pupils' relative experience with the objects and situations involved in the three types, respectively.

7. Building up interest in problems is best accomplished by having children work problems which are not too difficult and which represent genuine childhood situations. This is especially true in the case of pupils who are just at or below the median of the class.

8. The results seem to point to the conclusion that preferences for, or so-called interests in certain problems, are probably occasioned to a certain extent by the pupils' experiences which have been acquired,

and that they are influenced by whatever growth and changes occur in the experiences. This would suggest that, with respect to problem solving by junior high school pupils, preferences are not inherent and permanent qualities but are somewhat dependent upon estimated ability resulting from experience or acquaintance with the respective situations involved.

BACK-SLANTED WRITING AND SINISTRAL TENDENCIES

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A back-slanted script, or one which is slanted toward the left, has long aroused the curiosity of investigators. Graphologists characterize very definitely the writers of such script as showing defects on the emotional side, describing them in terms which the psychologist of today would probably interpret as descriptive of an introvert.

Apart from graphologists, the student of hand-writing finds such slant difficult to understand, since it is opposed to the hand-writing system taught in school and seems, in addition, an uncomfortable and tedious method of writing. It is a not uncommon belief that in many cases the writing of a back-slant indicates merely an affectation or the desire to be different on the part of the majority of writers of it, or that it is an imitation on the part of certain writers who take the suggestion from teachers, acquaintances or relatives whom they admire. On the other hand, penmen who write a back-slanted script frequently report that it is adopted to relieve a tension which is very fatiguing when writing is sloped forward.

In evaluation of systems of hand-writing taught in schools, some experimental work has been done relative to the slant of the most rapid and least fatiguing strokes of pen or pencil. Strokes slanted toward the right between twenty and thirty degrees from the vertical usually give the maximum of speed. Graves, on the contrary, reported from his experiment on six hundred four children that a backhand was the most rapid but least stable of all slants. He found it occurring, however, in the writing of only a small percentage of children.[1]

In general, the centrifugal movements of the hand and arms, that is, those out from the body as a center are more rapid, graceful, and less fatiguing than centripetal or those toward the body. Right-hand writing slanted to the right has been thought of as centrifugal or abductive, although it is obvious that considerable movement in handwriting must be centripetal or adductive. The normal writing

[1] Graves, S. Monroe: A Study in Handwriting. *Journal of Educational Psychology*, 1916, Vol. VII, pp. 483–494.

for the left hand is supposed to be mirror-script, which again is centrifugal in direction. Left-handed individuals who write from left to right labor under the disadvantages of a centripetal movement. In the discussion of the matter the relationship of slant of handwriting to handedness and to eyedness has not been raised with the detail that the subject deserves.

Having at my disposal a large number of specimens written with both hands, in both normal fashion and mirror-script, it has seemed to me worthwhile to summarize this material, with reference to the percentage of back-hand writing in differently constituted groups, and when possible, with reference to manual and ocular tendencies. The groups upon which I am reporting are the following:

1. Right-handed individuals writing.
   (*a*) Naturally with the right hand (centrifugally).
   (*b*) With the left hand for experimental purposes (centripetally).
2. Left-handed individuals writing.
   (*a*) With the left hand (centripetally).
   (*b*) With the right hand (centrifugally).
3. A highly selected group of supposedly dextral penmen chosen because of their production of a characteristically back-slanted hand and the possibility of obtaining information concerning their manual and ocular habits.
4. A miscellaneous group writing mirror-script for experimental purposes.
   (*a*) With the right hand.
   (*b*) With the left hand.

In the case of left-handed persons (Group 2) it is not possible to select groups that are strictly comparable to those arranged for the right-handed, owing to the presence among the sinistrals of the shift-over, or the individual who has been changed in writing from the preferred to the unpreferred hand. For this reason each of the two groups of sinistrals includes some practiced and some unpracticed left-hand writers. Such an overlapping is not found in the dextral group.

1(*a*). *Back-slanted Writing among Dextral Individuals Writing with Right Hand.*—Among three hundred seventy-nine specimens of writing by right-handed women I found from 4.5 per cent to 6.3 per cent producing either a consistently back-slanted writing or exhibiting considerable tendency to write in this fashion. The group of three

hundred thirteen men gave a slightly lower percentage, namely 2.5. It is my impression that reversed writing is less frequently found among men than among women. The total group of men and women—six hundred ninety-two all told—gave from 3.6 to 4.6 per cent of back-slant. The range of percentage indicates the difficulty in classifying handwriting as back-slanted or not because of variation from a uniformly reversed hand to the production of a variable and inconsistent back-slant. Occasional back-slanting of letters may be due to the position of the writing surface and other external factors.

Social and academic pressure operate so strongly to maintain the usual slant to writing that in many cases a reversal appears only under conditions of increased speed or when there is a removal of social pressure. I should expect more adults than young people to write a back-slanted hand for this reason. Graves[1] who presented figures on the slant of handwriting of six hundred four children who were thirteen years or under found back-slant in 2.4 per cent, an amount which would agree with my expectation. Graves does not, however, state what system of handwriting the children had been taught, possibly a matter of some importance in this connection. Crépieux-Jamin, the veteran graphologist, found in a given school in France,[2] in which vertical writing was taught, fifteen per cent of back-slant, which in part might be thought due to the excessive effort to avoid a right slant. In another school of adolescents from eleven to thirteen years, a school in which the children were allowed to choose their own direction, he found the extraordinary amount of 32.5 per cent back-slant. He attributes this excess to the instability of the adolescent years. Graves also remarks on the instability in writing of children entering the adolescent period, but finds it evidenced in mixed slant, rather than uniform back-slant. He found in his group 15.3 per cent who wrote a mixed slant. Possibly the difference between Graves and Crépieux-Jamin's figures is due to the criterion used. My procedure has been to classify as back-hand only a consistently reversed hand or one in which the drift to the left is outstanding. Moreover, my specimens are all from adult writers.

1(*b*). *Back-slanted Writing among Dextral Individuals, Writing with Left Hand.*—When we turn to dextral penmen who write with the left hand for experimental purposes we find a tremendous increase in the

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[1] Graves: *Loc. cit.*, p. 490.

[2] Crépieux-Jamin: "A B C de la Graphologie." Paris, 1929, p. 300f.

amount of writing that is back-slanted. A great difference also appears in the amount of back-slant dependent upon the conditions under which the writing is obtained. A group of one hundred twenty-three subjects wrote under experimental conditions for two minutes with each hand and under two different instructions. One specimen for each hand was obtained under conditions that stressed quality of writing. A second specimen under conditions that stressed speed of writing. Under such conditions 74.8 per cent produced a back-slanted script in either one or the other sample of sinistral writing. Usually both samples were back-slanted, but where this back-slant appeared in only one sample, that sample was the one obtained under instructions to speed.

A second experimental group consisted of four hundred eighty-four subjects who wrote their name and address with the left-hand in a questionnaire on handedness habits. Name and address written by the right hand appeared immediately above that written by the left. Under these conditions with a chance for imitation of the right-hand script, and with a very meager sample of left-hand writing, only a small percentage, comparatively, *i.e.*, 32.4 per cent, had recourse to back-slanted writing. The difference between 74.8 per cent in the first experiment and 32.4 per cent in the second clearly indicates the influence of speed, writing-interval and possibly absence of copy upon the amount of back-slanted writing produced by the left hand.

2(*a*). *Back-slanted Writing among Sinistral Individuals Writing with the Left Hand.*—One hundred forty-one sinistral subjects writing with the left hand produced 31.2 per cent back-slant or tendency to back-slant. There is a slight difference in this group between shift-overs (unpracticed with the left hand) and non-shiftovers (practiced with the left hand). The shiftovers give 38.8 per cent backslant; the non-shiftovers 22.4 per cent. This difference is probably not significant. If it is, it may be due to the fact that shiftovers who write habitually with the right hand have not had their left hand writing molded by conventional training.

The graphic specimens have been classified at face value. A number of left-handers, however, actually avoid back-slanting writing by giving their paper a peculiar tilt and writing over-hand or vertically. Such devious and peculiar methods of writing are worthy of special attention and are now subject to cinematographic analysis. In its extreme form we get here the interesting *Senkschrift* or downward writing. Several of my left-hand writers were known to employ a

partial *Senkschrift* on certain occasions and it is possible that some specimens used in the present tabulation were executed by this method.

Many left-handed writers produce an extraordinary medley of letters slanted variously. Some of them have been forced by teachers to abandon a comfortable back-slant and the result has been a most uneven and inconsistently slanted hand. Others, of course, write a fluent and consistently forward-slanted hand. Perhaps excessive effort is responsible for this latter achievement, as left-handed teachers frequently exhibit an excellent left-hand writing.

A group that should be studied with especial care consists of those individuals who write left-handed because of some early accident to the right hand. My collection here is too small for me to venture upon generalizations, but in one or two cases I have noticed pronounced tremors in these hands. In this connection a passage cited by Gould[1] is of interest. He quotes from J. J. Walsh, "Some So-called Rheumatisms," as follows: "One thing, however, has been much impressed upon me, and that is that those who are normally left-handed and are taught to write with their right hand, suffer from writer's cramp much more readily than normally right-handed individuals."

I have on record one or two cases in which individuals describing themselves as left-handed appear actually to have adopted this hand from some accident of imitation in childhood. Later they have been forced to shift to the right hand because of some form of cramp attacking the left! Critchley[2] believes *Senkschrift* is particularly serviceable for those who suffer from what he calls "obligatory sinistrality."

2(*b*). *Sinistral Individuals Writing with the Right Hand.*—One hundred forty sinistral subjects writing with the right hand gave from about twelve to fifteen per cent reversed slant with practically no difference between practiced and unpracticed hand. Recourse to printing is common in members of this group who write habitually with the right hand. Printing involves a drawing rather than a graphic movement and is, therefore, less difficult for them to maintain.

On the whole it is evident from the above figures that for adults there is a great increase in the amount of back-slanted writing when the left hand is used either natively or experimentally, this increase

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[1] Gould, George M.: Visual Function the Cause of Slanted Handwriting; Its Relation to School Hygiene, School Desks, Malposture, Spinal Curvature, and Myopia. *Medical Record*, April 22, 1905.

[2] Critchley, MacDonald: Mirror-writing. *Psyche Miniatures*, Medical Series, Kegan Paul, London.

being particularly noticeable under conditions that stress graphic speed. There is also a considerable amount of back-slanted writing on the part of naturally left-handed people who write with the right hand. Dextral back-slanted writing appears to be indicative of a drift toward the left.

3. *Supposedly Dextral Individuals Who Write a Reversed Hand.*— As part of the study of reversed slant a specially selected group was sought out. This group consisted of as many subjects as possible who wrote a consistently back-slanted hand. It was also necessary to limit the group to those who were available for testing of their manual and ocular habits. Forty-five of such individuals were obtained. All were supposed before examination to be dextral. Actual checking revealed a large amount of sinistral tendency. Four, possibly seven, proved to be actually shiftovers, that is, natively left-handed, others had mild unimanual preferences, that is, while they used the right hand as the preferred hand they were able to use the left hand with much dexterity. Three were ambidextral in the sense that the right hand was preferred for unimanual activities and the left for bimanual activities. A point of particular interest was the amount of left-eyedness in the group. Fully sixty per cent were left-eyed or had impartial vision, that is, labored under the disadvantage of crossed dextrality.

Since facts relative to eyedness were obtainable for the group of four hundred eighty-four dextral writers mentioned in 1(*b*), it seemed worthwhile to check this group also for crossed dextrality. Impure dextrals (RRL)[1] who were left-eyed gave nearly twice as much back-slant as did pure dextrals who were right-eyed. In part such slant insures better visual control.

It would seem that a consistently back-slanted writing throws suspicion on one's status as a pure dextral. But my group of forty-five included four or five individuals in whose case I was unable to locate any sinistral tendencies, exceptions which forbid dogmatic statements, or the possibility of using back-slanted writing in a differential way. There may, apparently, be more than one cause for back-slanted writing. It would be an interesting problem to investigate, whether any differences in appearance are to be found between the back-slanted writing of the pure dextral and the more usual sinistrally inclined writer of it.

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[1] Downey, J. E.: Types of Dextrality and Their Implications. *American Journal of Psychology*, Vol. XXXVIII, July, 1927, pp. 317–367.

4. *Mirror-writing Produced Experimentally.*—Mirror-writing, as is well known, is script which runs in a direction opposite to that of the usual hand, with a reversal of the individual letters. The sixty-seven specimens of mirror-writing that I used in the present investigation fell into three groups: (1) Thirty-five samples obtained for right and left hand respectively in a series of two minute tests of dextral and sinistral writing with each hand; (2) a series of twenty-five samples of writing of an alphabetical sentence in mirror-script with each hand; (3) a miscellaneous collection of seven specimens of a long verse executed in mirror-script with each hand.

4(*a*). Right hand mirror-specimens for these sixty-seven subjects showed back-slanting writing in 71.6 per cent of the cases, 9.7 per cent forward-slanting, and 18.6 per cent vertical writing or writing with a mixed slant. A forward-slanting writing in this case inclines towards the left, a back-slanting writing towards the right.

4(*b*). Left hand mirror-specimens for the sixty-seven subjects gave a back-slanted hand in 63.4 per cent cases; forward-slant in 17.1 per cent, 19.4 per cent vertical and mixed slant.

The interesting thing here is the immense amount of back-slanted writing both with the right and the left hand, that is, the prevailing slant is toward the right. This back-slant would be anticipated for the right hand since the writing here is centripetal and a drift back to the right might be expected both because of centrifugal habit and right-hand preference. The situation parallels that of writing dextral fashion with the left hand in which also a great amount of reversed writing was found. But why, one wonders, should left-hand mirror-writing give so great an amount of reversed slant or slope to the right? Possibly this is also due largely to dextral tendency.

Actually, however, the difference between right and left hand mirror-writing is much more pronounced than one would get from percentages indicating the slope forward or back. While 71.6 per cent of mirror-writing with the right hand is reversed and 9.7 per cent sloped forward, and with the left hand 63.4 per cent reversed and 17.1 per cent sloped forward, the degree of back-slant with the left hand is much less pronounced than with the right, and the forward slanting for occasional letters much more pronounced. Actual measurements of degree of slant were taken for extreme letters for the second series of twenty-five specimens. These measurements showed that there is some degree less slant toward the right with the left hand (perceptibly less for about seventeen of the twenty-five cases) but

that the outstanding difference is the inconsistent slant of the left-hand mirror-script and the number of individual letters forward slanted.

Left-hand mirror-writing has been thought by some to be the natural writing for all left-handed persons; by others, it has been thought to be the natural writing of the left hand only when the right hand has previously been trained in writing; it is due, *that is*, to symmetrical associated movements. In either case one might expect to find left-handed mirror-writing slanted toward the left, or in a forward direction. There is considerable evidence that it does so slant in naturally sinistral persons writing a spontaneous mirror-script. The prevailing drift to the right must then be interpreted as already suggested in terms of manual or ocular preference.

Of the sixty-seven specimens used in the above tabulation general handedness observations were available for the writers of Series 1 and 3, forty-two in all. These observations were, however, made some years ago so that there were no facts at hand for ocular dominance and little detail concerning the preferences of those who classify themselves as ambidextral. With these unfortunate limitations in mind, fourteen of the forty-two subjects may be listed as sinistral or possessing more than average sinistral tendencies. Five were left-handed, three were shiftovers, six considered themselves ambidextral. Some experimental material was at hand to support this latter classification. The subjects had been given steadiness tests, strength tests with both hands and had been required to write with both right and left hand, so that a standard comparison might be made of the quality and speed of left-hand writing. More than half of the forward slant in right and left hand mirror-specimens taken together was written by these fourteen sinistral or ambidextral subjects. One may put the evidence in another form by saying that of the fourteen subjects who were known to possess sinistral tendencies, nine showed to some degree a tendency to slant mirror-writing forward in either right or left hand writing, a tendency which is very rarely evident in unselected specimens. I believe that with larger groups and closer classification of the handedness of subjects the results would be much more conclusive.

One curious thing should, however, be remarked, namely, that the forward slanting was more evident in this group in ambidextral subjects and in shiftovers than in the left-handed who write with the left hand habitually. Possibly some graphic habit is masking the forward-slanting tendency in the case of the latter. In any case

graphic habit must be reckoned with. Two of the forward-slanting mirror-writers wrote a back-slanting right hand script, an interesting confirmation of what one would anticipate.

The most conclusive evidence, however, that the forward-slanting of left-hand mirror-writing points to native left-handedness is shown by the published samples of mirror-writing obtained from subjects who write mirror-script spontaneously. Beeley[1] has published "histories of ten typical cases of 'mirror-writing,' with facsimiles of their respective 'mirror-writings.'" All ten of these specimens slant forward, although in the case of two there is some tendency toward a mixed slant; that is, occasional letters slant backward. All of these writers wrote mirror-script spontaneously with the left hand. The tendency to mirror-write had, however, been suppressed to various degrees. Additional facts relative to the case histories and, in particular, information concerning ocular dominance might make it possible to differentiate even in these cases the significance of the degree of slant shown in the different specimens. The case studied by Carmichael[2] of a spontaneous writer of mirror-script also shows a forward-slanting specimen. Critchley[3] has reproduced a considerable amount of mirror-writing in his book "Mirror-writing." One very fine specimen (slanted forward) is reproduced from a patient suffering from a right-sided hemiplegia.

A reproduction is also given by Critchley of Leonardo da Vinci's mirror-script. The latter appears to one unacquainted with old Italian script to be largely vertical print, but with a slight tendency back. As there has been considerable controversy concerning the significance of the employment by Leonardo da Vinci of mirror-script, it might be well for experts to study in some detail the slant found in his mirror-written manuscripts. Was this mirror-script a spontaneous left-handed production? Or an artificial script induced by injury to the right hand or arm? Or a device to insure secrecy for certain of his manuscripts? The slanting tendencies might help in answer of these questions.

General Conclusions.—The fact that a small percentage of penmen, writing with the right hand dextral fashion, produce a back-slanted

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[1] Beeley, A. L.: An Experimental Study in Left-handedness. *Supplementary Educational Monographs*, Vol. II, No. 2, 1918, University of Chicago Press.

[2] Carmichael L.: Special Disabilities in Learning to Read and Write. *Harvard Monographs in Education* No. 6, 1925.

[3] *Loc. cit.*

hand raises the question of the general significance of slant to the right or to the left. It is found by experiment that a great percentage of penmen slant toward the left when writing with the left hand. Also that left-handed persons writing with the right hand produce a considerable amount of back-slanted writing. It would appear that slant toward the left is, in the first instance, indicative of a tendency to move centrifugally with the left hand; in the second instance, conditioned by sinistral tendencies as was also true for many supposedly dextral individuals writing a back-slant. Crossed dextrality likewise was found to be influential in causing a reversed script.

Sinistral writing or mirror-writing gave a large amount of back-slant with the right hand which was anticipated both because of centrifugal habit and dextral tendencies. It also gave considerable back-slant with the left hand where one would have expected a forward slant. Since a forward slant does characterize the spontaneous mirror-script of left-handed penmen, it would seem that dextral tendency is responsible for the frequency of back-slant in artificial mirror-script. Since the majority of people are right-handed it is not surprising to find the drift toward the right in sinistral writing more pronounced than toward the left in dextral writing.

There are some practical implications in the present investigation. Teachers may use a child's inclination to slant one way or another in connection with study of his manual or ocular preferences. There is a limit of comfort beyond which conventional slant should not be forced upon a child.

Graphologists should also take note of the relationship between slant and manual or ocular dominance. How can they justify their belief that slant is correlated with emotional trends? Unless, of course, they are able to establish a connection between handedness and temperament, which would be indeed a contribution to the psychology of personality. Even very irregular slant, which graphologists stress so greatly as evidence of temperamental instability, may result from conflict between muscular comfort and conventional training.

TABLE OF PROBABLE ERRORS FOR CERTAIN INTER-PERCENTILE RANGES

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With the increased knowledge of the proper use of statistical measures, the most elementary student of education and psychology is no longer content to use only some measure of central tendency in describing a distribution of measures; some measure of variability must also be given to aid in making the distribution meaningful. Though subject to a greater probable error, the quartile deviation is frequently used as a measure of variability rather than the standard deviation because of its ease of computation.

In many of the more recent standard tests, the tables of norms are given so that the score equivalent for a given percentile can be read directly from the table of norms. This type of table of norms is surely more valuable than one which gives in the table of norms only the median for a given age or grade. In the practical schoolroom situation it is at least as important and likely more important for a teacher to know and compare the range of achievement of her pupils than to know their average achievement. It might be well to mention at this point that test makers should include in the table of norms a detailed description of the population used in obtaining the norms and should give the entire distribution of scores obtained rather than giving only the mean or median for each age or grade.

Present indications are that inter-percentile ranges will be used more frequently in the future than they have been in the past. It is often advantageous to compare the range of the middle one-third of a certain distribution with the middle one-third of another distribution, or to compare the middle seventy-five per cent of one distribution with the middle seventy-five per cent of another distribution. In order that such a comparison be meaningful, it is necessary to have some measure of the probable error of these inter-percentile ranges. The authors have found it very convenient to have at hand a table from which could be read a value which, when multiplied by the particular inter-percentile range, gives the probable error of that inter-percentile range.

Kelly (1) gives the following formula for the standard error of an inter-percentile range whose limits are equal percentile distances from the ends of a normal distribution:

$$\sigma P_p - P_{(1-p)} = \frac{\sigma}{z\sqrt{N}}\sqrt{2p(q-p)} \tag{1}$$

In order that the use of the terms p and q may be consistent throughout this article and conform with their use in the K-W tables (2), Kelly's formula has been rewritten as

$$\sigma P_q - P_p = \frac{\sigma}{z\sqrt{N}}\sqrt{2q(p-q)} \tag{2}$$

in which q is the lower of the two percentiles and p the higher. It must also be remembered that p is the same percentile distance from the upper end of the distribution as q is from the lower end of the distribution.

By use of the Kelly-Wood Table (2), we and that σ can be found in terms of the inter-percentile range. For example, the inter-percentile range $P_{10} - P_{90}$ is equal to $2x\sigma$ in the K-W Table or 2.563104σ. Then since the inter-percentile range $P_{10} - P_{90} = 2x\sigma$, $\sigma = \dfrac{P_{90} - P_{10}}{2x}$. It will be noted that the inter-percentile range is written $P_{90} - P_{10}$ in the preceding equation. Inter-percentile ranges are commonly written with the lower of the two percentiles given first, as $P_{10} - P_{90}$, or, in the general terms here used, $P_q - P_p$. Strictly speaking, since P_q is smaller than P_p, $P_q - P_p$ gives a negative quantity. So when in the formulas in this article the actual distance between the points P_q and P_p is meant, the terms are written $P_p - P_q$ since it is P_p minus P_q that is desired.

Assuming a normal distribution, we can, therefore, substitute for σ in the formula the inter-percentile range divided by $2x$ (read from K-W Table). Thus we have

$$\sigma P_q - P_p = \frac{\dfrac{P_p - P_q}{2x}\sqrt{2q(p-q)}}{z\sqrt{N}} = \frac{\sqrt{2q(p-q)}(P_p - P_q)}{2zx\sqrt{N}} \tag{3}$$

Then,

$$\mathrm{PE}_{P_q - P_p} = \frac{.6744898\sqrt{2q(p-q)}(P_p - P_q)}{2zx\sqrt{N}} \tag{4}$$

$$= \frac{.6744898\sqrt{2q(p-q)}}{2zx\sqrt{N}}(P_p - P_q) \tag{5}$$

Letting

$$A = \frac{.6744898\sqrt{2q(p-q)}}{\frac{2zx}{\sqrt{N}}} \tag{6}$$

Then

$$\mathrm{PE}_{P_q - P_p} = A(P_p - P_q) \tag{7}$$

VALUES OF A

Number of Cases

Inter-percentile range	30	40	50	75	100	125	150
5 –95	.1089	.0943	.0843	.0689	.0596	.0533	.0487
10 –90	.1095	.0948	.0848	.0693	.0600	.0536	.0490
12.5–87.5	.1126	.0975	.0872	.0712	.0617	.0552	.0504
15 –85	.1168	.1011	.0904	.0738	.0640	.0572	.0522
20 –80	.1280	.1109	.0992	.0810	.0701	.0627	.0573
25 –75	.1436	.1244	.1113	.0908	.0787	.0704	.0642
30 –70	.1654	.1433	.1281	.1046	.0906	.0810	.0740
33 –67	.1831	.1586	.1418	.1158	.1003	.0897	.0819
35 –65	.1977	.1712	.1531	.1250	.1083	.0969	.0884
37.5–62.5	.2207	.1911	.1709	.1396	.1209	.1081	.0987
40 –60	.2516	.2179	.1949	.1591	.1378	.1233	.1125
45 –55	.3714	.3216	.2877	.2349	.2034	.1819	.1661

Number of Cases

Inter-percentile range	200	250	300	400	500	1000	2000
5 –95	.0422	.0377	.0344	.0298	.0267	.0189	.0133
10 –90	.0424	.0379	.0346	.0300	.0268	.0190	.0134
12.5–87.5	.0436	.0390	.0356	.0308	.0276	.0195	.0138
15 –85	.0452	.0404	.0369	.0320	.0286	.0202	.0143
20 –80	.0496	.0443	.0405	.0351	.0314	.0222	.0157
25 –75	.0556	.0498	.0454	.0393	.0352	.0249	.0176
30 –70	.0641	.0573	.0523	.0453	.0405	.0287	.0203
33 –67	.0709	.0634	.0579	.0501	.0448	.0318	.0224
35 –65	.0766	.0685	.0625	.0541	.0484	.0342	.0242
37.5–62.5	.0855	.0764	.0698	.0604	.0541	.0382	.0270
40 –60	.0975	.0872	.0796	.0689	.0616	.0436	.0308
45 –55	.1438	.1287	.1174	.1017	.0910	.0643	.0455

In the accompanying table the values of A are given for certain values of N and for certain inter-percentile ranges. An example of the use of the table is given here.

Find the PE of the inter-percentile range $P_{35} - P_{65}$ in the distribution in which P_{35} is 42.6, P_{65} is 74.8, and N is 40.

The difference between P_{65} and P_{35} is 32.2 in scale units. Reading from the accompanying table, when N is 40 and the inter-percentile range is $P_{35} - P_{65}$, the value of A in the formula is .1712. Then $PE_{P_{35}-P_{65}}$ in the particular distribution is .1712(32.2) or 5.5126. If the value of N in the example were 500, then $PE_{P_{35}-P_{65}} = .0484(32.2) = 1.55848$ or 1.56.

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AN AGGRESSIVE CHILD IN A NURSERY SCHOOL

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Occasionally at two years a child may have all the self-assertive characteristics of an aggressive person. The child's behavior at this early age may so closely resemble a dominating adult that it is difficult to understand how in so short a time he developed such definite "adult" traits. The children react to him as adults react to a similar type of adult. They respect his power to lead and control them, but they are not gentle and kind to him. They are glad to hurt him and to band against him. It appears that all the fundamental urges and desires that characterize adult society are already developed in the society of the small child.

Behavior patterns are contagious. When an aggressive child enters the nursery school the problems of all the children in the school are accentuated because the group imitates the behavior of the most dominant child. He sets the pattern which is adopted by all the others, so that the school soon finds itself faced, not with one child who bites, but with six children who bite; not with one case of grabbing toys from others, but a dozen such cases.

If the aggressive child is allowed full expression of his wants, the other children may suffer. The question of adult interference arises at this point. How far should adult standards of behavior be imposed on young children? The outcome is generally an attempt to curb the aggressive child or to redirect his behavior to more social and less personal advantage.

At the Washington Child Research Center in Washington, D. C., we made a study of an aggressive child and her effect on the group. This paper describes the behavior of this child at entrance to school, her previous history, the methods used to redirect her activity, and her subsequent behavior. It is hoped that perhaps from this discussion we can glean some reasons for the development of a dominating personality, and also what we can do to guide it to useful maturity.

Description of Child at Entrance to School

Our first contact with Ann was in February, 1929 when she entered the nursery school. She was a pale thin little girl of twenty-seven

months. She had a low threshold of response to any stimulus; that is, she reacted exceedingly promptly to noise or disturbances. This caused her to be more easily irritated by the interferences of other children and by the necessity of having to conform to certain school routine. Physical examination, however, revealed her to be a normal well-developed youngster, although slightly underweight. Her intelligence quotient was average for the school group.

Ann looked docile and demure, but we soon learned that she was buoyant and energetic. She was seldom still, but was bouncing, shouting, or interfering with someone continually. It was a job to keep her occupied. She not only accelerated activity at school but apparently had a repertoire of antics at home. Her parents stated in the initial history that she was "too full of mischief" and that they were having difficulty with her. She had refused to take a nap the two months before entrance to school and had ripped the bed to pieces every time she was put to bed. During each meal at home she was troublesome because she ran away from the table many times and she took food which was not meant for her. The mother also stated that she dominated other children and grabbed things from them, although her playmates were older than she. In spite of this her playmates were fond of her. The parents' main concern at this time was to gain obedience without the use of physical force.

When she entered the school she would run away from the lunch table as many as ten times during the meal. She would take food from the plates of others. She would shout, talk, or sing in the nap room just at the moment when the teacher in charge thought the twelve children were ready to drop off to sleep. Ann always did these things with one eye on the adult to see what reaction she would get and at the same time tried to arouse a response from the children. It was as though she wanted to assert herself over adult authority by changing the tone or attitude which the adult had established. For example, if the adult insisted on quiet in the sleep room, Ann wanted noise; when the adult wanted a composed peaceful lunch hour, Ann wanted excitement and attention. She would gain it by antics, a sudden shout, a somersault, or even by knocking down a child.

In spite of her mischievousness Ann was a charming child. She was gentle and considerate to the younger children and probably made them happier the first days of school than anyone else. She "mothered" them, led them around, played with them, and protected

them from less thoughtful children. More than one little child feeling lonely and strange was made laughing and gay by a simple game of Ann's invention. Furthermore, not only the children loved her during these flashes of originality, but the adults themselves were devoted to her. In spite of her many annoyances, her unexpected actions were always refreshing and amusing.

Home Environment

The most unusual point in Ann's history was her birth to parents of a late marriage. As the parents were considerably older than the average parents (father—47, mother—37), they may have been less flexible in their adjustment to an addition of a child in the home. They were probably less matter of fact with her than a child born to younger parents. She was given not only the attention of her mother but that of her father, a retired business man who was home a great deal. In fact, after a baby sister arrived when Ann was twenty-four months old, Mr. Brown took care of Ann.

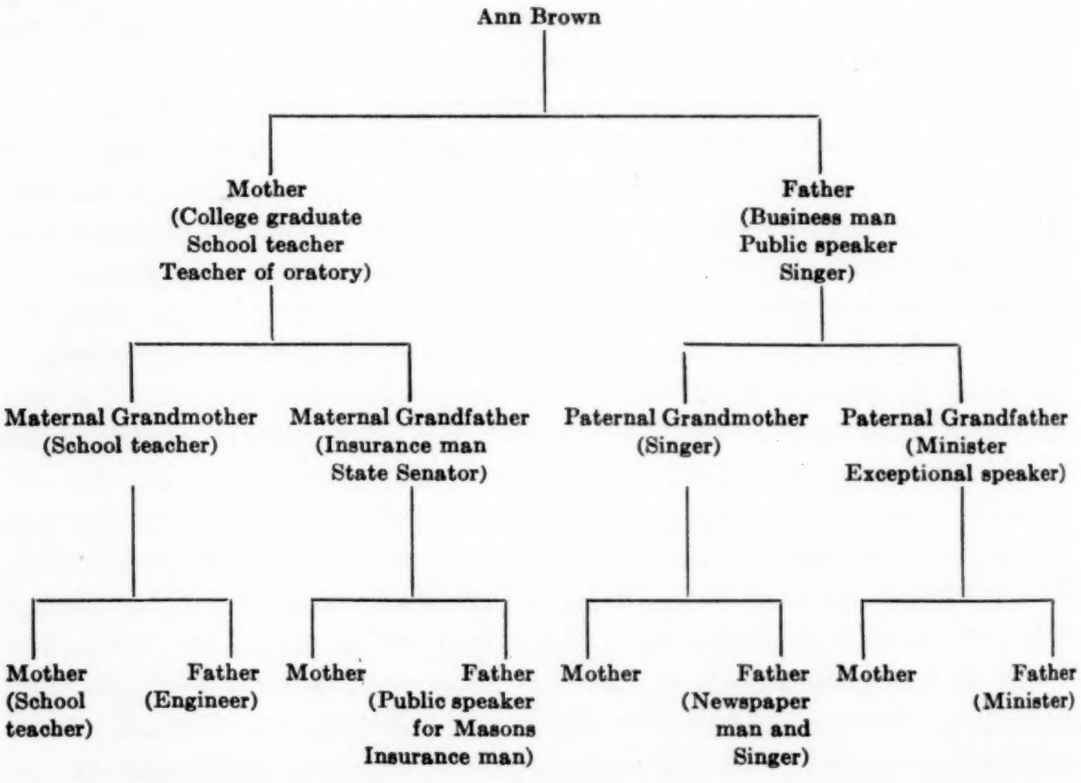
Mrs. Brown is a quiet complacent woman. Her most outstanding characteristics are her orderliness about herself and her home, her friendliness, and her intelligent interest in her children. Mr. Brown, on the other hand, is a big energetic jolly man who was amused at everything Ann did. He has a deep bass voice which brought instant obedience when on rare occasions he used a stern tone. Ann probably relied on his good nature and made a game of repeating a mischievous act until he became annoyed by it. Repeated annoyances to which Ann subjected her family crystalized into the behavior difficulties which her family complained of at Ann's entrance to school.

Both parents are college graduates and have numerous interests outside of the home. Mr. Brown sings in a choral society and Mrs. Brown gives dramatic readings. They do many outdoor things together, such as gardening, swimming, and picnicking. Their life is well-balanced and mature and quite obviously has not been planned as a stage on which Ann was to direct the players. She did have excellent indoor and outdoor play spaces, but her importance in the home did not impose on the normal life of the adults. However, there was nothing in the attitude of the parents toward Ann, nor in the physical set-up of the home, to indicate the need for her to be aggressive in order to gain prestige. There was no problem between Ann and her sister, for they seemed well adjusted to each other.

Heredity

In earlier years, before we believed so thoroughly in the dominance of the influences of environment on personality over those of heredity, we might have had a simple explanation for Ann's behavior. Perhaps the interest in dramatic play, the desire to direct and to change the trend of others' play, is based on some innate tendency. At least there seems to be evidence which could be so interpreted, although it is impossible to determine what characteristics of her parents she has adopted through contact with them in the home rather than through inheritance. There have been orators, senators, singers, and actors in her family for three generations.

The following table gives the careers of various ancestors. Only the parents, grandparents, and great grandparents are charted. One paternal uncle is a college professor and another a minister. A great uncle was an actor.



Of the fourteen parents and grandparents, three were school teachers, one an engineer, one a senator, two ministers, and four public speakers or singers. In other words, of her fourteen ancestors, the only ones who did not have careers were three great-grandmothers, while eleven did public work demanding certain aggressive personality

traits. These characteristics were either inherited or acquired by each generation on both sides of the family.

School Methods of Treatment

Regardless of the cause of Ann's personality, the school had the problem of definitely redirecting her desire to dominate to channels constructive for the development of the other children and desirable for her own development. She needed to learn that in order to live with a group her own desires must sometimes be subordinated to the needs of the group. Quiet in the nap room, respect for property, and sharing of equipment were some of the customs she had to learn.

The method the school used to redirect Ann's activity was to minimize the satisfaction she received from misbehavior by paying little attention. Instead, the teachers gave her endless opportunities during the day to dominate situations in the proper way. An effort was made to praise her for kindnesses to other children so that she would learn that approved behavior brought attention to her. She was encouraged to do things for the group advantage, such as making beds or setting tables. After she was adjusted to the school routine she was expected to conform to the simple school order like any other child. If she persisted in leaving the lunch table it was assumed she was through eating, and her food was removed; and if she persistently interfered with other children she was isolated from them. She was made to understand what was expected of her before there was any question of discipline. This program was consistently adhered to, and yet in such a manner as not to antagonize her.

We overcame one child's personal dislike for Ann by planning to have the father take Ann and the little friend on several excursions after school. This gave the child a pleasant feeling toward Ann and really helped Ann to change her tactics with the group as a whole, for before this the group banded against her to prevent her from playing with them. Toward the end of the year it was a common occurrence to see Ann sitting with a book telling stories to a half-dozen children. Her comments and methods of controlling the group were identical with those of the teacher, as shown by recordings of her conversation. She would have the children's complete attention for several minutes at a time.

In order that the teachers would be in absolute agreement as to the treatment of Ann, frequent staff conferences were held to discuss her development and any needed revision in her treatment. Thus

an effort was made to maintain as much stability in the school treatment of Ann as was possible. Frequent conferences were held with her parents to discuss Ann's problems at home and at school. Detailed plans were worked out to help carry the work of the school over into the home. For a period of two weeks the child was dropped from school in order to study her behavior at home. During this period the teachers visited in the home to observe Ann's behavior there and to help the parents gradually to put into use, in the home, methods found most satisfactory in the school.

During the two weeks at home the parents were instructed in the taking of verbatim records. Thus one of the parents each day recorded the child's language and activity during any random fifteen minutes of her play. When these records were studied along with similar records made by the teacher at school it was noted that the child's method of directing play was the same in the home as in the school.

The records taken in home and school also made an excellent basis for discussing the child's problems with the parents. They likewise served to show the results of the treatment, when one record was compared with another taken several months previously.

At intervals during Ann's fifteen months attendance at school other records were kept, by which her play interests could be studied. These showed that when she first entered school she spent most of her time on the swing, playing with wheel toys, and trying to direct younger children. At the end of fifteen months she spent her time almost exclusively in playing "house" or imaginative games with several children at a time.

After the child had been in the school fifteen months we decided to give her a special mental test besides that generally used in the school, and also to give her a vocabulary test and several performance tests. Our aim was to ascertain if she had certain abilities and development advanced for her age. If so, this might explain her desire to dominate a group her own age. Some facets of her development might not be recognized through the use of one test while they might through another.

According to the Madorah Smith test for the extent of vocabulary in young children, Ann had a vocabulary of 1400 words, which is average for a child her age. The following table gives the results of the various other tests, all made when the child was forty-four months old. The "mental age" (MA) indicates the age at which an average

child would obtain the development shown by the child being tested. The "intelligence quotient" (IQ) is simply $\frac{\text{MA}}{\text{actual age}}$, and therefore measures the extent to which the child's development is faster or slower than average.

SCORES ON SOME MENTAL AND PERFORMANCE TESTS
(Age of Child 44 Months)

Names of tests	MA in months	IQ
Mental tests:		
Binet (Stanford Revision)	54	123
Kuhlman	51	116
Performance tests:		
Stutsman	48	109
Goodenough (drawing a man)	51	116
Baldwin and Stecker (selected tests)	42	95
Gesell (drawing tests)	48	109

It is seen that on tests from a Performance Scale Ann had an IQ above the average. On these performance tests she showed excellent motor coordination, speed, imagination, and a fine visual and auditory memory; but from these tests, as well as from the Stanford and Kuhlman, it was found that she is limited in abstract intelligence. She rates high in sociability and initiative, and is very flexible in her adjustment to social situations. She translates thought into action immediately, but she lacks precision and sufficient planning, as shown by the high number of errors where an exact performance is expected.

RESULTS OF WORK WITH CHILD

As we look back over the months of work with this child we see the gradual changes in Ann's behavior. She is still an aggressive, domineering individual, but she has learned how to stay within the bounds of individual rights. During her first year in school her originality, sense of humor, and dramatic ability gave her great prestige over the others, but when the novelty of these wore off a group of children would often ostracize her from their play by ignoring her or using physical force to keep her away. These heartaching experiences caused her to cry, sulk, or interfere with the children to express her unhappiness over not being wanted in their play. This was a difficult

time in her adjustment, and if it had not been for the sympathy and understanding of the head teacher the child might not have made so splendid an adjustment. She gradually learned, however, the need of being tactful and tolerant of others' ideas if she desired to play with them. She no longer knocked children down, grabbed their toys, or forced them to do as she wished. Instead, she substituted verbal means to make her ideas seem unusual and attractive, and to hold her place as leader. Her methods of domination had been changed from a physical to an intellectual basis.

The development of tact and tolerance has been a definite gain through the year's experience, but the permanence of it depends on the parents. They are the stable factor in her life and on them rests the responsibility of seeing that she does not slip back into old modes of behavior as she proceeds through her school career.

Conclusions

When Ann came to us she was definitely a dominating individual having all the unpleasant characteristics of a tactless executive. Her contact with the nursery school and the deliberate efforts which were made there to shift her personality to a more socially-acceptable form, have succeeded, temporarily at least. But whether she continues to progress in this direction depends on her subsequent environment, both at home and in school.

One broad general conclusion can be drawn from this study, and that is, that it is possible to adjust a child with a dominating personality to a school group without suppressing or destroying her ability and desire to dominate. A school, however, is not the only place where such an adjustment can be made. The problem can equally well be treated in the home, through the use of methods similar to those of the school. In the home just as in the school, the child can be taught that though he has rights within limits, so do others in the social group, and that each should respect the rights of others.

The child must have plenty of good opportunities to express his desire to direct others. At the same time he must appreciate his own limitations, learn to tolerate the ideas of others and to follow as well as to lead. None of these things are taught quickly but can only be achieved by patient work of both parents and teachers.

WHAT IS SCIENTIFIC?

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The object of this discussion is to suggest some degree of tolerance among scientists.

Science! Science! It is heralded from our house top, wafted on the air. It is echoed in thousands of laboratories and college halls, uttered by pupils in our grade schools and discussed by housewives and street cleaners. Our magazines are filled with it. To be outside the pale of science is to be condemned, to be lost! That would not be so bad if we could only decide when we are *scientific.*

We are no longer satisfied with science in mathematics, astronomy, chemistry, physics, medicine, and biology, but its domain has been extended into education, literature, the arts, morals, history, and religion. Havelock Ellis would even have us inject science into love-making, and Judge Lindsey would carry it into marriage. We have a recent medical prediction that science will in time regulate all desires and passions of mankind. Let us hope for the control of scientific intolerance. Everything must be scientific, even to charity and ideals. But the term *scientific* needs some treatment itself. The legislators of several states have undertaken the job, but I am skeptical about the outcome being valuable either to the fundamentalists or to the cause of clearer thinking.

In what sense can the word scientific or science be used in order to justify this expansion? Is it possible to preserve the original mathematical quantitative notion of science and thus apply it to all that now goes under the head of research? Can and must all human experience be reduced to a quantitative basis? If we were content to define science as Herbert Spencer did, "Partially classified knowledge," we would at once recognize an indefinite number of varying degrees of the scientific. This conception of scientific variability would give us a more decent respect for those who labor in fields where classification is difficult and where it must be on a different basis. We might even coin some new terms to designate our relative procedures. At least we might limit the term *scientific* to purely quantitative considerations and apply the much heralded *research* to qualitative inquiry.

Most of our controversy arises from the fact that we are trying to apply science to phases of human experience demanding a *qualita-*

tive as well as a quantitative consideration. How can history and literature ever be scientific in a quantitative sense, to say nothing of morals? Quality implies evaluation for which there is no objective standard. We may speak of one thing being better than another in the sense that it produces a given result in a shorter time and with less expenditure of energy. But as soon as we say one thing is better in the sense that it satisfies a human desire, a human need, or helps to reach the end of our strivings, all objective quantitative standards disappear.

Yet it is in this subjective court of appeals that all standards of good and bad, justice and injustice, right and wrong, beauty and ugliness, happiness and misery, morality and immorality, religion and civilization are formed and stored up. These standards rest on the desires, needs, and cravings of the human heart, without which nothing, not even life itself, would have value.

Is it not clear then that scientific accuracy in a quantitative sense becomes impossible in proportion as we are dealing with the complexity of these introspective cravings of mankind? Shall we, therefore, pronounce all forms of research that fail to reduce their results to mathematical tables and graphs, unscientific? Must psychology, for example, abandon all forms of introspection or continue to be designated as *unscientific?* Thirty years ago the social sciences started on the road to mathematical accuracy and much has been accomplished, but when the sociologist makes no suggestions concerning *relative values* of the different lines of activity or fails to offer some plan for future betterment, he will certainly fail to meet the demands of society. He makes the fatal error pointed out by Mr. Hyde, "of thinking that the measurable is identical with the significant." This I take to be the underlying, most significant statement of Mr. Hyde to sociologists and psychologists, in his valuable article on, *Can Science Control Life?* We shall return to it again.

Let us admit that mathematical accuracy should be the goal wherever possible, but let us not think lightly of the great endeavors that lie beyond quantitative science even in the subjective field of human values. *So long as there is an honest effort to secure all the facts possible, to apply them to man's needs, and so long as love of truth means more than victory, the procedure is praiseworthy as being scientific.*

I wish to show that at least four factors will always vary the degree of scientific accuracy. These factors are (1) The possibility of securing the truth, (2) The degree in which subjective standards must

be used, (3) The degree in which control conditions are possible, and (4) Emotional bias. Certainly some of the bitter accusations about not being scientific will disappear if these factors are given their proper attention.

1. *The Possibility of Securing the Truth.*—This problem confronts us in many of our most important fields of research. Some thirty-five years ago G. Stanley Hall began his laudable and persistent effort to secure facts concerning the psychological aspects of sex. Up to that time all had been dogma and guess work, and most of it still is. Physicians and research organizations have been putting forth every possible effort to collect reliable facts. But even these supposed facts cannot be relied upon with any degree of scientific accuracy because the almost universal tendency to conceal and to misrepresent this side of life makes most of it uncertain. The physician and social worker meet a certain class of people, and the psychologist and psychoanalyst another. They may have much in common, but they often do not. Again we have no basis for comparing such psychological facts with similar conditions of the past. The cloak of religion and secrecy has been thrown over this side of human conduct. So here is one field where scientific accuracy will probably remain limited.

Let the sociologist attempt a scientific treatment of divorce and see if he will fare any better. He will probably tabulate a thousand cases where mental cruelty is stated as the cause of divorce, not ten per cent of which is true. Or, suppose any religious foundation should attempt to ascertain scientifically the number and per cent of Unitarians in any given community in the United States. What are the prospects for scientific accuracy? Who does not know Unitarians who bear the name of Methodist, Presbyterian, Catholic, or Mormon?

Counting sex delinquencies, divorces, and Unitarians is not the same as counting units of energy, atoms in chemical compounds, and money values in different countries. Even the much desired mathematical graphs of sex delinquency and divorce must be interpreted differently. There is no model Unitarian and even if there were, there is no means of comparison or of ascertaining in what degree one is a Unitarian. These are legitimate fields of research because human relations are of supreme importance. We should not belittle the men who labor in fields where facts are difficult or inaccessible by calling them unscientific. If we think it impossible to bring such worthy endeavor under the *scientific* then let us classify it all as *humanistic research.*

2. *The Degree in Which Subjective Standards Must Be Used Will Modify the Degree of Scientific Accuracy.*—Here we are face to face with the dispute that had divided the science of psychology into opposing groups during the last fifteen years. The behaviorists tell us that nothing is scientific unless it is done in a laboratory under controlled conditions and free from the contaminations of introspection. All must be objective and measurable. Consequently any emotion of love or religion must be described only in terms of its physical manifestations of muscular movements, contractions, temperature, circulation, and glandular secretions. But we shall see later that even this *physiology* lacks scientific accuracy. With this class of thinkers any consideration of suggestion, imitation, instinct or of the subconscious, which implies any underlying mind force as a cause is tabooed as unscientific and as loaded with blind mysticism. I think we must admit that introspective psychology can never become scientific in the sense of mathematical accuracy. Subjective standards of good and bad, pleasure and pain, work and play, love and hate, beauty and ugliness, will never be more than approximately the same for any two individuals. But are all attempts to secure this approximation to be discouraged, abandoned and pronounced worthless? By whom? Why? Humanity has not, and by the very nature of things never can contemplate objective conditions except in terms of subjective desires, aims, and purposes. Even if the universal mechanism of the behaviorists is true, humanity will probably go on acting as if desire, striving, aim, and purpose are of fundamental importance.

Is not this inner experience a legitimate object of scientific research? Have not the outstanding changes of the last fifty years in regard to the causes of human conduct been produced chiefly by the study of subjective forces. For example, what would instinctive forces have meant, if it had been possible to have described the child's conduct only in terms of his physical activities without any reference to such subjective feelings as fear, anger, love, hate? If we had not made the connection between these physiological states and the mental and emotional life we would still be dwelling in our old theological views of human conduct. How can we ever plan for human needs unless we know at least approximately how people *feel* and *think* about this great subjective world? Who can decide what is worthwhile and what are the great longings of mankind? We admit our knowledge to be quite inadequate but there is no reason to be disheartened.

We should all come to realize that these two fields supplement each other. In the field of human conduct, the subjective aspect is necessary to complete the meaning of the objective. The behaviorist properly held up to ridicule the extent to which psychology once attributed to animals, even the lower organisms, a mental life similar to our own because of a similarity in conduct. But is it not equally unscientific to ignore or deny any mental life back of such activities when we absolutely do not know anything about it? I am well aware that this form of analogy is dangerous and has been carried to extremes. But all sciences, even the physical, rely upon this method to some extent. Biology makes extensive use of it and archaeology and anthropology are almost solely dependent upon it. Could any one live among a people, even the lowest savages, whose language he did not understand, without attributing to them certain mental and emotional qualities on the basis of the similarity of their actions to his own?

In spite of the difficulty of being scientifically accurate, psychology will one day become the most practical of all sciences because it will discover what the chief needs, desires, and cravings of man are and the best means to satisfy these fundamental longings which constitute the object of living and of all human strivings.

When psychology secured a divorce from philosophy, the antagonism became so great that the psychologist has ever since scorned philosophy. As a result few present day psychologists show any knowledge even of the philosophical problems to which their facts naturally lead. On the other hand the modern philosopher not only knows psychology but present day philosophy is founded upon psychology and biology. It is this failure to get a glimpse of life in its totality that limits and nullifies many of our psychological interpretations. Recent investigations show the part played by interest, attitude of mind, motive, and deep-seated purpose. The presence or absence of these intellectual conditions modifies all learning, memory, reason, endurance, and accomplishment. Even fatigue seems to be due in a large measure to the absence of interest. None of these qualities can be studied by the objective method alone.

The climax of the objective quantitative tendency was reached during the use and development of intelligence tests. Emotional make-up, deep-seated desire, interest and purpose were left out of consideration. In many places teachers were hired on the basis of intelligence tests, and pupils were promoted or demoted on their IQ in spite of the judgment of the teacher who lived with them and of the

psychological effect on them. Happily there is a wholesome reaction and a tendency to consider these results only as one of many factors. To temper out expectations to where we anticipate the discovery of *general tendencies* only, is one thing; but, if we expect absolute and invariable laws, we are doomed to failure.

Conditions are still worse when we read into these general tendencies that mathematical accuracy characteristic of the physical sciences, and then proceed to determine education and human destiny accordingly. Hundreds of books and all manner of wild speculations have been built upon the results of the army tests administered during the World War. It seems strange that these writers never stop to think that the experiment was new to almost every one, that most of those who administered these tests were inexperienced, that different desires and emotional states modify results, and that the temptation to manipulate results for certain ends was very great.

As an example of building on these sandy foundations take that book of Professor Murchison, on "Criminal Intelligence." One is amazed to find that he has simply compared these should-be-forgotten army tests with tests of some of our criminal population. He thereby finds a high degree of intelligence among criminals. Or look at Mr. Wiggam's "The Fruit of the Family Tree" and other writings in which he concludes from the army tests that only four per cent in America have any brains. We even forget that these standards are arbitrary and subjective. We might fix on another standard that would make most of us geniuses and still others that would make most of us idiots.

3. *The Degree of Control over Conditions Will Vary the Scientific Accuracy.*—In experimenting with living organisms it is impossible to be sure we have control of all the conditions. This would still be true even if we ignore the fact that no two living organisms are exactly alike. Even though you have control today, tomorrow the organism has changed. So far as we know atoms do not accumulate different habits to modify their reactions in the same manner as animals and children do. None at least of the higher organisms are ever the same after an experiment as before; and differences in previous experiences prevent any two from responding exactly alike.

Thus, the behaviorist or any one else who prides himself on absolute scientific accuracy in measuring responses, reflexes, respiration and circulation is doomed to some degree of failure. His conditions and standards are shifting all the time. Years ago I added numbers for thirty consecutive days at the same hour. However, in spite of all

efforts to have conditions the same, at no two times was I the same individual either physically or mentally. Besides that I was accumulating disgust for such boredom about which I lied apparently with success. But either the other eleven victims were not disturbed in the same way, or they too joined the Ananias Club. However, on account of past experience and tempermental conditions no two of us could have been influenced alike. The much extolled *learning curve* and a thousand other forms of inquiry in our public schools, for similar reasons, must all stop short of scientific accuracy. For example, when we conclude that the *rate of learning* between six and forty years of age follows a given curve, on what grounds can we claim scientific accuracy? We do not select a thousand children of six and take their learning curves straight through each year to forty. But usually a different group is used each year. How do we know that the thousand children examined at six years would show the same rate of learning as another group if both were examined at eighteen? However, suppose we should take the rate of learning of any thousand children each year from six to forty, would we then have scientific accuracy? By no means, for at best forty per cent of the original group are dead. Did the best or the poorest learner die?

I am not even hinting that these investigations are not all quite worthwhile. But I do mean to say most emphatically that if *scientific* is to mean mathematical quantitative accuracy these fields of research where control-conditions are so impossible can never be scientific. However, if one does not parade his results as absolute laws, and uses all possible care to eliminate error and to secure and interpret facts, should his results not be called scientific until we find some other name by which to label them? Why should scientists in other fields sneer at such worthy efforts?

4. *Lastly, Emotional Bias Modifies Scientific Accuracy.*—Think of the many fields of knowledge into which this emotional bias creeps either openly or in such an insidious form as to deceive even the best of investigators. It often dominates in politics, in national relations, in religious problems, in historic inquiry, in literary judgment. It poisons the results in educational and social research and makes the individual sin against himself whenever research is required as a means of graduation or promotion.

In dealing with educational, social and religious conduct few of us can free ourselves from the dominance of personal desires and deep-seated beliefs. We feel that things *ought* to be so and so. I confess

that I am one of those who believes that this dominance may be partly or wholly unconscious. Even politicians understand this and act upon it in the confirmation or rejection of judges of the Supreme Court. But the interesting thing is that the senators in voting were under similar influences to the one they so abhorred. In emphasizing the place which rationalization plays in human thinking Robinson's "Mind in the Making" rendered a real service. James had already employed the idea in his "Will to Believe." In many ways we accumulate reasons to hide the real *reason* for our conduct and beliefs. Many investigations are made to justify previous convictions or some intended future action.

Remember that when we are dealing with human relations and human rivalry we have introduced a powerful factor largely unknown in building a bridge, discovering a new chemical element, or sounding the depths of the sea. Whenever mind is pitted against mind we have a different problem.

To turn our attention to the part which personal ends and personal gain play in defeating scientific accuracy is no pleasant task. Thousands of students are now engaged in some line of research on which their graduation and future positions depend. What a strain on human nature! In fields of great uncertainty and of unstable conditions thesis-making may flourish. But in many cases common sense would have told anyone where the research would come out. Thesis-making is becoming the most dangerous disease of higher education.

Here let us return to that statement of Mr. Hyde—"The measurable is not identical with the significant." In the first place what is the significance of most of these researches on the ability of these graduates to teach high school boys and girls? Or what would be the significance of a thesis on the Sex Complex of Milton to anyone? In every large school where thesis-making is carried on there should be those individuals who have some large grasp of life as a whole and whose duty it should be to guide students to the more significant.

To look upon new facts as valuable in themselves is dangerous. Psychology has accumulated a vast number of facts about rats, a large number of which have no more significance that the number of grains of sand in a cubic foot on Daytona Beach would have. Scholarly enthusiasm comes not from thesis-making but in pursuit of a great cause pregnant with significance. The young researcher needs high-minded guidance to the end that he may avoid useless and evident investigations.

However, the evil is wide spread. The Carnegie Foundation carried on an extensive investigation concerning high school athletes and reported to us that a larger per cent of them go to college than of other boys and that in college they make an average record. Did we need an investigation to tell us that? Both facts are evident to any one acquainted with school life. More recently we have an investigation concerning corruption in athletics about which there seems to be a diversity of opinion. The results of many other investigations in various fields were either evident in advance of time or the personal desires of the investigators directed the collection of facts.

In other cases personal gain and antagonism play a detrimental part. Who does not look with skepticism upon some of our educational surveys? A special expert is brought two or three thousand miles and paid one thousand dollars and expenses to give a critical estimate of your school system. At the same time an electrician is in a similar way brought to the same town by a business firm interested in the possibilities of a great power plant. What difference would you expect in the reports of these two men?

Historical research will always consciously and unconsciously suffer from the limitations of human bias and prejudice. There have been many efforts to reduce history to a science, especially that of Henry Adams, only recently so ably presented by James Adams in the "Yale Review." Here we have the suggestion that we need to "depersonalize" historic characters by the use of symbols such as are employed in algebra and chemistry. Even if this were possible, how could the historian become "depersonalized"? No historian of the World War can free himself from subjective conditions that influence his interpretations. Mr. Bury who made a praiseworthy effort to reduce history to a science claimed that history like other sciences should be treated as an end in itself. However, he later admitted that "human knowledge has no value out of relation to human life."

In conclusion, I wish to say that I have faith in science and in the scientific method. But I am only reacting against an over-passionate worship of mathematical scientific accuracy; against the failure to recognize varying degrees of possible scientific accuracy; against the condemnation and discouragement of those who work in fields demanding a different method; against a worship of figures, graphs, and curves that become blind to humanity. I believe that we must view life and knowledge in its totality and not in isolated parts.

Who knows but that tomorrow we may be beyond what we know as scientific today? The real problem today is not so much to bring work down to a given scientific level as it is to expand science into higher fields to meet the needs of human life and to look beyond science to that which may now be conjecture, guess, or philosophical speculation. No scientific discovery has value except in terms of human desires, needs, aims and happiness. Which is the greater discovery radio or the typhoid fever germ? No possible answer can be given until you begin to think of the pleasure, education and convenience radio gives, and of the safety to human life on the other hand. All *evaluation* is subjective. Let us not forget that even the physical sciences show marked tendency to the *subjective.* To quote from James Adams in "Yale Review": "Whatever scientist one reads now, above the level of the mere laboratory experimentalists and observers, who are mostly twenty years behind present thought, one is continually reminded of the subjectivity of the whole scientific structure. In this respect, men so different in many ways as Bridgman, Eddington, Whitehead, and others would seem substantially to agree."

Finally, there are three ways by which we may arrive at a better understanding of scientific endeavor. We may take a broad view of the whole field and recognize the varying degrees of possible scientific accuracy. We might if we like, divide the vast field of scientific inquiry into different groups according to the method used and the nature of the problems involved. Or the greatest psychological lesson humanity has yet to learn is that, if any of us were placed in the same situation as those we criticize, we would follow similar lines to those we so distrust. This is true of practically all individuals and true of nations.

RESULTS OF AN EXPERIMENT IN PREDICTING SUCCESS IN FIRST YEAR ALGEBRA IN TWO OAKLAND JUNIOR HIGH SCHOOLS

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In the Spring semester 1929, a request was received from the Assistant Superintendent in Charge of Secondary Education to try out the Orleans Algebra Prognostic Test:

1. With a view to ascertaining whether or not this test would be useful in selecting pupils who should be encouraged to take beginning algebra and

2. To work out a plan for its use in the junior and senior high schools of Oakland if the results seemed to justify it.

A number of principals volunteered to assist in the experiment, and the assistance of the principals in two junior high schools was accepted.

The counselors in these schools were requested to permit all those who desired to take first year algebra to enroll in the classes, so that as nearly as possible an unselected group might be obtained. During the first week of the Fall semester 1929, the Orleans Algebra Prognosis Test was given to all of the first year algebra pupils in these two schools, thirty-eight in School No. 1 and ninety-eight in School No. 2.

At the close of the Spring semester 1930, the Columbia Research Bureau Algebra Test was given to the same pupils who took the prognostic test in the Fall, seventy-one pupils remaining in the class at School No. 2 and twenty-nine at School No. 1. Of the thirty-six on whom complete data were not available, some left town, some transferred to other schools, and some were permitted to drop the subject. It is to be regretted that the scores made by these pupils could not have been included in the summary, but the nature of the experiment made this impossible. Intelligence quotients, obtained from the Terman Group Test of Mental Ability, were available on all pupils and were included on the record form.

The various partial and multiple correlations were computed for the total group of one hundred pupils, and separately, for the seventy-one pupils from School No. 2. The results are reported for the latter computations only, as the variation in the teaching factor would, and did, lower the correlations when the results from the two schools were

TABLE I.—SCORES MADE ON ORLEANS PROGNOSTIC AND COLUMBIA RESEARCH BUREAU TESTS IN ALGEBRA IN JUNIOR HIGH SCHOOL NO. 2 SCHOOL, YEAR 1929–1930

Pupil No.	IQ	Score in prognostic test	Score in achievement test	Pupil No.	IQ	Score in prognostic test	Score in achievement test
1	130	173	22	37	123	109	10
2	121	125	22	38	115	141	10
3	126	94	17	39	115	130	9
4	147	135	17	40	105	62	9
5	133	142	17	41	105	84	9
6	133	165	17	42	129	124	9
7	119	93	16	43	99	73	8
8	121	131	16	44	106	72	8
9	83	83	15	45	100	72	8
10	101	117	15	46	108	101	8
11	134	131	15	47	108	82	8
12	113	142	15	48	99	106	8
13	136	136	15	49	126	108	8
14	119	146	15	50	119	91	7
15	108	140	14	51	104	91	7
16	115	130	14	52	122	118	7
17	119	105	14	53	116	102	6
18	107	146	14	54	104	96	6
19	125	106	14	55	116	92	6
20	116	108	14	56	105	120	6
21	129	149	14	57	101	100	6
22	99	106	13	58	99	77	6
23	117	124	13	59	102	112	5
24	120	93	13	60	100	73	5
25	93	127	12	61	110	113	5
26	91	87	12	62	84	57	5
27	132	107	12	63	103	59	5
28	60	87	12	64	89	76	5
29	106	91	12	65	86	84	4
30	134	116	12	66	102	96	4
31	107	116	11	67	99	130	4
32	123	93	11	68	106	92	4
33	109	134	11	69	109	88	3
34	112	89	11	70	105	98	3
35	111	107	11	71	108	78	1
36	113	89	10				

combined. The same teacher taught all of the pupils, whose scores are reported in this school, and the teaching factor may therefore be considered to have been held constant.

The scores made on the two tests and the intelligence quotients for the same pupils are given in Table I. The pupils are ranked in order of scores made on the achievement test.

The work sheet showing the computation of the various constants is given below.

(1) Achievement Test Score	(2) Prognostic Test Score	(3) Intelligence Test Score
$M_1 = 10.2$	$M_2 = 106.7$	$M_3 = 111.3$
$\sigma_1 = 4.58$	$\sigma_2 = 25.1$	$\sigma_3 = 15.1$
$r_{12} = .61$	$r_{13} = .48$	$r_{23} = .50$
$PE_{r_{12}} = .06$	$PE_{r_{13}} = .07$	$PE_{r_{23}} = .07$

$$\overline{X}_1 = .11X_2 + 9$$
$$\sigma_{(est.\ X_1)} = 3.62$$
$$PE_{(est.\ X_1)} = 2.4$$
$$r_{12.3} = \frac{.61 - .48 \times .50}{\sqrt{1 - .48^2}\sqrt{1 - .50^2}} = .49$$
$$r_{13.2} = \frac{.48 - .61 \times .50}{\sqrt{1 - .61^2}\sqrt{1 - .50^2}} = .26$$
$$r_{23.1} = \frac{.50 - .61 \times .48}{\sqrt{1 - .61^2}\sqrt{1 - .48^2}} = .30$$
$$\sigma_{1.23} = 4.58 \times .7924 \times .9656 = 3.50$$
$$\sigma_{2.13} = 25.1 \times .8660 \times .8717 = 18.95$$
$$\sigma_{3.12} = 15.1 \times .8660 \times .9656 = 12.63$$
$$b_{12.3} = .49\frac{3.50}{18.95} = .09$$
$$b_{13.2} = .26\frac{3.50}{12.63} = .07$$
$$x_1 = .09x_2 + .07x_3$$
$$\overline{X}_1 = .09X_2 + .07X_3 - 7.2$$
$$\sigma_{(est.\ \overline{X}_1)} = 3.50$$
$$PE_{(est.\ \overline{X}_1)} = 2.36$$
$$R_1(23) = \sqrt{1 - \frac{12.25}{20.98}} = .65$$

It will be noted that the correlation between the prognostic test scores and the achievement test scores is $.61 \pm .06$. That this test

has some predictive value will be evident when it is seen that the coefficient of alienation is reduced from 1.00 to .79.

The regression equation for predicting the achievement test score of a particular pupil from his prognostic test score is $\overline{X}_1 = .11X_2 - 9$ where $\overline{X}_1$, is the predicted achievement test score, X_2 the score on the prognostic test, and 9 a constant. The probable error of estimate of this predicted score is 2.4.

As an example of the use of this regression equation, the records of several students from the class are given below.

Pupil No.	Score on prognostic test	Estimated score on achievement test	Actual score on achievement test
2	125	$\overline{X}_1 = (125 \times .11) + 9 = 23$	22
3	94	$\overline{X}_1 = (\ 94 \times .11) + 9 = 19$	17
9	83	$\overline{X}_1 = (\ 83 \times .11) + 9 = 18$	15

The above cases are merely illustrative of the use of the equation, and it is not to be expected that the correspondence would be as close in unselected cases.

The correlation between intelligence quotients and achievement test scores is $.48 \pm 07$. This is lower than is usually found, and a repetition of the experiment in this or in other schools would doubtless give a larger value.

The three partials were next computed and are interesting but not pertinent to the particular problem under consideration.

The regression equation for predicting the achievement test score in terms of intelligence and prognostic test scores is $\overline{X}_1 = .09X_2 - .07X_3 + 7.2$, where $\overline{X}_1$ is the predicted achievement test score, X_2 the score on the prognostic test, X_3 the intelligence quotient, and 7.2 a constant. The probable error of this estimated score is 2.36.

The use of this equation is illustrated by the records of several students as shown in the table on p. 313.

As was the case above an unselected group would show a much wider divergence between predicted and obtained scores, and those chosen are merely illustrative of the results to be expected under the most favorable conditions.

The multiple correlation is .65, and with this value the standard error of estimate has been reduced twenty-four per cent. This should

Pupil No.	IQ	Prognostic test score	Estimated achievement test score	Obtained achievement test score
6	133	165	$\overline{X}_1 = .09 \times 165 + .07 \times 133 - 7.2 = 17$	17
11	134	131	$\overline{X}_1 = .09 \times 131 + .07 \times 134 - 7.2 = 14$	15
21	129	149	$\overline{X}_1 = .09 \times 149 + .07 \times 129 - 7.2 = 15$	14

be of appreciable advantage in school administration and is well worth the effort involved.

Summarizing the scores in a somewhat different manner the following points may be noted:

Of the eighteen pupils who obtained the highest scores on the prognosis tests, thirteen were in the highest quarter of the distribution of the achievement test scores, three in the second, one in the third, and one in the fourth.

Of the eighteen pupils who obtained the next highest scores, three were in the highest quarter in achievement, eight in the second highest, five in the third, and two in the fourth.

Of the eighteen pupils who obtained the next highest scores on the prognosis test, two were in the highest quarter in achievement, five were in the second highest quarter, four in the third, and seven in the fourth.

Of the lowest seventeen pupils on the prognosis test, one obtained a score in the highest quarter on achievement, two in the second highest quarter, seven in the third, and seven in the fourth.

The matter may be considered from another angle. The authors of the Columbia Research Bureau Algebra Test have found the lower quartile of the scores upon which the test was standardized to be twelve; in other words seventy-five per cent of these pupils made scores equal to or in excess of twelve. If this test is a good test, and it is so considered, a pupil whose true score lies below twelve has not profited, as far as achievement is concerned, to any considerable extent from the year spent in the study of algebra.

If this prognostic test were given to those pupils who wished to enter first year algebra and the regression equation given above solved for the members of the class, those pupils whose predicted scores obtained from the equation were seven or less would have three

chances in one hundred of reaching the twenty-fifth percentile in the Columbia Research Bureau Algebra Test, and they could well be strongly urged to take other courses and omit the algebra.

In conclusion, it should be noted that the above results apply only to this particular junior high school and should be computed for each school desiring to use the test. However, the labor is not excessive and in view of the fact that algebra is a college preparatory subject and is intended primarily for those pupils who expect to enter college, it might be desirable to select more carefully the pupils who are permitted to enter this course.

BOOK REVIEWS

The Measurement of Interests, by Douglas Fryer. New York: Henry Holt and Co., 1931. Pp. XXXVI + 488.

In writing a book on psychology there is always a considerable temptation to try to do two things at one time: Interest the scientist and appeal to the lay reader. Mr. Fryer, recognizing the impossibility of such a feat, confines himself to the task of presenting with scholarly exactness the significant studies so far made in the field of interests. The result is a volume satisfying to the scientifically-minded—a not too common event in these days of popular psychology.

Reports of nearly all of the investigations are accompanied by descriptions of the techniques used. Though it is true that the descriptions tend to overbalance the interpretations, the reader will be glad that Mr. Fryer has made possible a checking of experimental procedures. When a generalization is offered, there is little doubt as to its justification. A few such generalizations follow:

> Interests of today are based in part upon past interests, and interests of the future are determined to a measurable degree by interests of the past. That interests have some degree of permanence is unquestionable.
>
> Interests and abilities are different qualities in mental life. Neither one is dependent upon the other. Neither can be predicted from the other.
>
> Interests are primarily of environmental origin.
>
> Extreme lack of vocational interest and extreme variety of disconnected interests may be expected more frequently from the neurotic and badly adjusted.

As the author himself says, "The objective measurement of interests is only begun; it is hardly out of the experimental stages." A few workers with Mr. Fryer's scientific insight could soon improve the situation.

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The Scientific Basis of Social Work, by Maurice J. Karpf. New York: Columbia University Press, 1931. Pp. XVIII + 424.

The Director of the Training School for Jewish Social Work is plagued by the question, "Has social work a scientific basis?" He realizes the work cannot receive professional recognition until that question can be answered in the affirmative. The author has examined the literature of social work in general, and family case work in particular, and has sifted the available evidence. He has asked the

following questions: (1) "What knowledge do social workers need?" (2) "What knowledge do social workers use?" (3) What knowledge do social workers receive?" (4) "How may social work acquire a scientific basis?"

In Part I, Karpf presents an analysis of the social work literature, and quotes liberally to show the wide range of knowledge that has been thought necessary for the education of a social worker. Scant agreement has been reached by the "authorities." Strong cases have been made out for teaching Anthropology, Biology, Economics, History and Philosophy, Political Science, Psychology, Sociology, and Social Research. The evidence presented by the author indicates quite clearly that the leaders in social work are concerned with the problem of placing their work upon a professional basis by providing a sound background of general, or cultural education upon which to superimpose scientific training that will enable them to react to their work situations in an objective manner. They have not reached agreement, however, concerning the methods of accomplishing that result.

A scrutiny of one hundred family case records showed that fifty-five per cent of the judgments expressed were concerned with attitudes, sentiments, emotional states, appearance, personality and personality traits, health and disease. A tabulation of the words used in describing temperament, volition, character and intelligence was made. It is clear that the terms used in family case records are loosely used, and that their meanings shift as they are employed by different workers. We know that unverified estimates of general intelligence are unsatisfactory in describing an individual. The same must be equally true when a case worker attempts to describe the personality traits of a patient.

After discussing the views of the authorities in the field of social work regarding the training requirements of workers in the field, and analyzing family case records to discover what types of knowledge appear to be used in describing clients, Karpf made a study of the admission requirements and curricula of schools of social work. He concludes, "From the standpoint, then, of giving students such preparation as we have been told social workers should have for the development of accuracy of thought and expression, it may be doubted whether the schools are doing an adequate job."

Karpf recognizes five basic needs of the adequately trained social worker. These are (1) knowledge of the backgrounds of the people

with whom the social worker is working, (2) a sound and wholesome philosophy of life and the social point of view, (3) a knowledge of norms so as to be able to recognize deviations from an accepted standard, (4) a knowledge of human nature, and (5) accuracy of thought and expression. He feels that "the knowledge necessary for social work can be obtained largely through the study of the social, psychological and biological sciences."

Karpf's "The Scientific Basis of Social Work" creates the distinct impression that an admirable job has been accomplished *in assembling current data and opinions upon the problem of training social workers.* It seems to this particular reviewer, however, that the problem of *selection of individuals to be trained for social work* must receive primary consideration before the work can be put upon a scientific basis.

Perhaps it is futile to attempt to put such work upon a professional plane with the personnel that it attracts at present. It might be well to examine the intelligence level and background of social workers in order to determine whether they are capable of taking the type of scientific training that the leaders feel is necessary. Since they often are called upon to aid in the solution of emotional difficulties, it might be well to inquire into their own emotional stability. Is the unmarried worker qualified to advise patients regarding the intimate problems of married life? Training of social workers cannot be considered adequately until the problem of selection is faced squarely. The systematic selection of intelligent, physically fit, well integrated, socially minded individuals would aid materially in putting social work upon a professional basis.

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Diary of a Child's Life from Birth to the Fifteenth Year, by Vilhelm Rasmussen. Translated from the Danish by Margery Blanchard. London: Gyldendal. Pp. 187.

Professor Rasmussen gives in this diary the observations which he and his wife made of their younger daughter. The diary is a record of the child's life from birth to her fourteenth birthday. Instead of using the notes to revise his previous works on psychology, the author says in the preface: It is my hope that the method of presentation here employed may encourage parents who take an interest in child psychology to keep diaries similar to mine; for the greater the material in the form of careful observations that is once available, the easier

it will be to discriminate between those traits in a child's development which hold good generally and those peculiarities which distinguish the individual child.

It is possible the text suffers from the unavoidable difficulties of translation particularly in the record of language growth. But if these obstacles were overcome, the Diary would still fall short of its aim. The outstanding weakness is a lack of precise definition of "those peculiarities which distinguish the individual child." An example is a paragraph (p. 5) under "Faddiness." Other similar topics are: Activity, movement, religion, investigation, etc.

This defect centers attention upon the purposes which such diaries can serve for the psychologist. Quantitative measures of physical growth are more reliable than single measures for one child. The physical measures included will be valuable to the extent which they may influence individual peculiarities. It is more in the realm of attitudes, habits, and their origins, in personality peculiarities, that trained observers may contribute through detailed diaries. Professor Rasmussen has done a service in this direction by keeping his diary. There are many interested and intelligent parents in our own country capable of keeping such diaries. It remains for the psychologist to determine the pertinent events to record (a list obviously requiring constant revision as our knowledge increases) together with provision for proper interpretation of the records. This is a field full of promise for parent groups and child study clubs especially where the services of a school psychologist are available.

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Education as a Social Force—As Illustrated by a Study of the Teacher-training Program in Nova Scotia, by M. V. Marshall, Cambridge, Massachusetts: Harvard Studies in Education, No. 18, Harvard University Press, 1931. Pp. XI + 161.

The aim of this study, as the author states, is to illustrate how the educational institutions of an area, particularly those for training teachers, can be intimately related to the life of the people. More specifically stated, the problem is that of finding out what the present conditions of providing trained teachers in Nova Scotia are and of formulating a plan based on a consideration of these findings. The method of studying the problem consists, first, of setting up criteria for

evaluating a teacher-training program; and, secondly, of evaluating the present provisions for training teachers in Nova Scotia in the light of these criteria and the social background; and, finally, of proposing a constructive program for training teachers in the province. With this problem in mind, the author briefly describes several criteria to which an adequate teacher-training program should conform. These are derived from a study of the theory of education and the prevailing and past practices in a number of the leading countries of the world. In evaluating the present provisions for training teachers in Nova Scotia, Mr. Marshall is careful not to confuse the irrelevant facts with those which aid only the analysis and evaluation of the present teacher-training practice.

Perhaps the most outstanding feature of this monograph is the careful analysis which the author makes in establishing valid criteria for evaluating an effective teacher-training program. His technique in this respect seems adequate, and should prove valuable to those interested in making similar studies in the field of education.

Another important phase of the study is the constructive teacher-training program which the author proposes. His proposals are freer from the customary weakness of such studies. As far as the reviewer could determine, the proposals are in harmony with his findings.

The study, as a whole, should provide a challenge to the educational leadership of Nova Scotia. Its method and findings are suggestive of more effective and objective ways of attacking educational problems.

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Henry Barnard on Education, by John S. Brubacher. New York: McGraw Hill Book Co., 1931. Pp. X + 298.

Comenius, by M. W. Keatinge. New York: McGraw Hill Book Co., 1931. Pp. VIII + 255.

Pestalozzi, by Lewis Flint Anderson. New York: McGraw Hill Book Co., 1931. Pp. X + 283.

These three books are all members of a series known as the McGraw Hill Education Classics, edited by Edward H. Reisner, of Columbia University. They are bound in uniform style and serve, each in a special field, a uniform purpose. Each volume makes available to students of the history of education the most outstanding writings of an outstanding man of an earlier educational era. In each case

the compiler of the volume has prepared an introduction on the life of the man and on the place of his works in educational literature, and has written the necessary introduction to the different selections in order to provide the right viewpoint on the part of the reader and to compensate for the portions which must necessarily be omitted.

The volume on Henry Barnard will be of greatest interest to students of American education, not only because of our natural interest in the man and his influence on education but because in his writings we see a picture of the whole panorama of American education during the nineteenth century. The volume is far more than a personal study; it is an excellent source-book on American education, in which we get Barnard's keen sense of values and his warm missionary spirit, both expressed in the writings which he addressed to his fellow citizens.

The book on Pestalozzi contains selection from "Leonard and Gertrude," from "How Gertrude Teaches Her Children" and from each of the other major writings of this great forerunner of modern education. The book on Comenius is primarily an abridged edition of "The Great Didactic." Sample pages from different textbooks which Comenius prepared for school children are reproduced near the end.

As a series, and as individual treatises, these educational classics are valuable publications.

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